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CONDUCTED BY

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AND

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"Nec aranearum sane textus ideo melior quia ex se fila gignunt, nec noster vilior quia ex alienis libamus ut apes." JUST. LIPS. *Monit. Polit.* lib. i. cap. 1.

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PLATES.

ERRATA.

Page 51, line 9 from bottom, *for* Muswell *read* Muswell Hill.
Page 73, line 25, *for* disilicate *read* bisilicate.
Page 151, last line, *for* Herculis *read* Hercules.
Page 169, line next above the equation (12.) *for* b' *read* b.
Page 171, second line of (18.) *for* A_3 *read* A'_3.
Page 174, line 10, *for* correctly *read* approximately.

Erratum in Vol. xiii.

Page 469, line 5 from the bottom, *for* paralysed *read* polarized.

THE

LONDON AND EDINBURGH

PHILOSOPHICAL MAGAZINE

AND

JOURNAL OF SCIENCE.

[THIRD SERIES.]

JANUARY 1839.

I. *Chemical Examination of the Fire Damp from the Coal Mines near Newcastle. By* [*the late*] Edward Turner, *M.D., F.R.S. London and Edinb., V.P.G.S., Professor of Chemistry in the University of London.**

THE gases subjected to examination were collected under the direction of Mr. Hutton, by emptying Winchester-quart bottles filled with water, at the spot where it was designed to collect the gas, and then inserting a well-greased ground-glass stopper, which was afterwards secured in position by cement, and a covering of bladder. About half an ounce of water was left in each bottle, and the bottles were sent to me packed in boxes in an inverted position. In most instances, when the stoppers were withdrawn in the pneumatic trough, a portion of water instantly rushed in, showing both that the means of securing the gases had proved effectual, and

[* From the Transactions of the Natural History Society of Northumberland, Durham, and Newcastle-upon-Tyne, vol. ii. Part ii.] From the period of its institution the Natural History Society had directed particular attention to the evolution of gas in coal mines, and many papers had been read from time to time, when the feelings of the public were most painfully excited to the subject by the awful calamity at Wallsend Colliery, on the 18th of June, 1835, described by Mr. Buddle in the preceding paper [in the Society's Transactions]. At this time an inquiry was in progress before a Committee of the House of Commons, which soon after published its report. It was given in evidence before this Committee, that both free hydrogen and olefiant gas occur in the atmosphere of some coal mines; this, as striking at once at the efficacy of the Davy lamp in preventing explosions, seemed to be a matter requiring immediate attention in a district where that instrument is so extensively used, and where its safety is so entirely relied upon. With this view, immediately after the publication of the Parliamentary Report, the Natural History Society determined to institute such an inquiry, and Mr. Hutton was directed to communi-

that the gases within the mine were in a more rare state than in my laboratory.

As one of the principal objects of the inquiry was to determine in how far the gas of different mines varied in chemical constitution, it was material to multiply as much as possible the samples of gas submitted to examination. The number of samples actually received and examined by me amounted to twelve. The result of this analysis will be given in a tabular form at the close of this communication. The general conclusion deducible from them is, that the essential and sole inflammable material of fire-damp, as formerly found by Dr. Henry and Sir Humphry Davy, is the light carburetted hy-

cate with the committee of the coal trade to ask their valuable co-operation and assistance; for this purpose he addressed the following letter to Robert William Brandling, Esq., the chairman.

Copy.

"Newcastle, January 9th, 1836.

"Sir,—I beg leave respectfully to state, that at the last meeting of the Natural History Society, after the reading of a paper on the gas of mines, it was resolved that the Society should do all in its power to promote an investigation into the nature of the gas evolved in our different collieries, for the purpose of ascertaining if any other, and what gas, occurs besides the common carburetted hydrogen, it having been stated in evidence before the late Parliamentary Committee, that free hydrogen and olefiant gas are both to be found in the mines of Wales. I was directed by the Society to bring the matter before the Committee of the Coal Trade, and request their valuable co-operation and assistance in obtaining an extensive analysis by one of the first chemists of the day, so as at once to set at rest the question as to the nature of coal gas spontaneously evolved in this district. Dr. Turner was named as the person best fitted for the task, not only from his great skill as an analyst, but from his extensive knowledge as a geologist, and the attention he has paid to the chemistry of nature, so to speak.

"This investigation will not be an expensive one, and it was thought, from the deep importance of the question as connected with the safety-lamp, that the Coal Trade would have no objections to join the Society in the cost. The Society are anxious that this investigation should be made speedily, as they are about going to press with a conclusion of the second volume of their Transactions, where they would wish this to appear as forming an appropriate Appendix to the many valuable papers in the work connected with our local geology and mining. If the Committee of the Coal Trade agree to give their assistance in this matter, the Society will furnish them with any number of copies of the results of the investigation they may require; and, individually, I most respectfully beg to offer my personal services in collecting the specimens of gas, and making such arrangements as will secure their conveyance to London unadulterated. "I have the honour to be, &c. &c.

"William Hutton, Secretary.

"To Robert William Brandling, Esq."

The Coal Trade Committee immediately adopted the suggestion, and appointed John Buddle, George Johnson, and Nicholas Wood, Esqrs., to make the necessary arrangements for collecting the specimens of gas.

drogen, or marsh gas of chemists, which issues in a state of purity from coal, wholly free from admixture with hydrogen, carbonic oxide, or olefiant gases, and but rarely containing a trace of carbonic acid gas*. The sole difference in the explosive gas of different mines must hence be referred to the degree of admixture with air. If diluted with nineteen or twenty times its volume of air, the mixture does not detonate or take fire at all; on diminishing the proportion of air below this term the mixture becomes inflammable, and on the approach of a lighted candle, a pale blue flame appears, which passes slowly through the mixture when the air is in large excess; rapidly when the ratio is favourably adjusted for combustion. The most explosive mixture, as Davy correctly states in his "Essay on Flame†," is formed of one measure of pure fire-damp, and about seven measures of air. Such mixture, unlike an explosive mixture made with air and hydrogen or carbonic oxide gas, is not kindled by incandescent solid matter, such as a mass of hot iron; but it burns rashly [rapidly?] in contact with flame, and detonates readily with the electric spark. As the proportion of pure fire-damp rises above a sixth, the mixture burns less and less readily, and the tint of the flame changes at the same time from blue to yellow or brown. The phænomena receive a ready explanation from the well-known principles established by Davy.

The analysis of fire-damp was performed by detonation with oxygen gas over mercury. In successful analysis with all the gases, the diminution in volume subsequent on detonation with the electric spark, and due to gaseous matter condensed as water, was precisely twice the volume of carbonic acid gas which was generated, and equal to the oxygen gas which disappeared. The volume of carbonic acid gas sometimes fell short of half the diminution due to production of

* Extract of a letter from Major Emmett, Royal Engineer, to Mr. Hutton, dated Hull, 19th February, 1836.

"I send you the following extracts from a letter from Dr. Dalton of the 13th. As regards Wallsend Pit, they are important, and to me conclusive. I sent him three bottles Mr. Buddle had collected for me about three months ago, also one of water from the old working at Gateshead Park Pit, forwarded to me by Mr. Wood. Respecting the Wallsend gas he says: 'I received your letter and bottles of gas safely, and soon after opened the bottles under water. The air in each bottle was very much alike. It was constituted of some two or three per cent. of carbonic acid, about one tenth common air rather short of oxygen, and the rest, about eighty-five per cent., was pure carburetted hydrogen, or pond gas, without a trace of either pure hydrogen or olefiant gas.' Respecting the Gateshead water he says, 'The bottle of water from the old waste I also examined; it contained about one per cent. of soluble matter, chiefly common salt, with some carbonic acid, sulphurous acid, sulphuretted hydrogen and lime.'"

[† See Phil. Mag. First Series, vol. xlxvi. p. 448.]

water, but this only took place when the combustion was incomplete. Sometimes the gaseous mixture, after detonation, was more or less obscured by a deposit of carbonaceous matter, and in such instances, as already remarked by Dr. Henry, there is always a deficiency of carbonic acid gas, which deficiency is less considerable the more completely the mixture at the moment of detonation approximates to perfect transparency. I have occasionally observed this cloud, even when ample oxygen for complete combustion was present; but with a decided excess of oxygen it generally does not occur at all, or at most in so slight a degree as not to be appreciable. To show the course of the inquiry, I quote three analyses, in the first of which an error, from deposited carbon, is apparent.

I. *Analysis of fire-damp from Jarrow Colliery, which issued from a seam of coal eleven fathoms below the Bensham seam.*

Specific gravity as found by weighing the gas = 0·6209. Tested by nitrous gas it was found in one experiment to contain 2·25 per cent. of oxygen, and in a second 2·1 per cent., indicating as a mean 2·2 per cent. of oxygen, equivalent to 11 per cent. of air. This gas, which was quite free from carbonic acid gas, may be considered as a mixture of 89 measures of real marsh gas with 11 measures of air. A gas so constituted, and assuming 0·5595 as the specific gravity of marsh gas, should have a specific gravity 0·6079; for 0·5595 +0·89 + 0·11 = 0·6079. Of this gas 12·3 measures, containing 0·3 of oxygen and 11 of real marsh gas, were fired with 32·7 measures of a sample of oxygen gas, which contained 31 of real oxygen gas:—

Loss due to condensed water	= 22·3
Carbonic acid gas generated and absorbed by potassa	= 9·4
Residual oxygen, determined by firing with hydrogen gas	= 10·5
Deducting 10·5 + 9·4, the oxygen above accounted for, from 31·3, the whole oxygen gas originally present, there remain, as Oxygen gas which went to the production of water ...	= 11·4

II. *Analysis of a gas from the Bensham coal seam, Jarrow Colliery, collected from a blower, which caused the accident in* 1826.

Specific gravity actually observed = 0·6381.

This gas was quite free from carbonic acid gas. In two

trials with nitrous gas, it was found to contain 3·7 per cent. of oxygen, equivalent to 18·5 of air. A gaseous mixture of 18·5 air, and 81·5 real marsh gas, should have a specific gravity of 0·641, since 0·5595 + 0·815 + 0·185= 0·641.

Of this gas 13·5 measures, inferred from the foregoing premises to contain 0·5 of oxygen and 11 of real marsh gas, were fired with 30 measures of oxygen, which contained 28·8 of real oxygen gas.

Loss of volume due to production of water	=	22·8
Carbonic acid gas generated	=	11·2
Residual oxygen	=	6·4
Deducting 17·6 from 29·3 there remain, as Oxygen gas which went to the production of water	=	11·7

III. *Analysis of a gas from the Eppleton Jane Pit, Hutton Seam, Hetton Colliery, collected at a depth of* 175 *fathoms below the surface.*

Specific gravity actually observed .... = 0·78.

This gas was quite free from carbonic acid. Two experiments with nitrous gas agreed in indicating the presence of 4·6 per cent. of oxygen, equivalent to 23 measures of air. Analysis indicated the presence of 50 per cent. of real marsh gas, leaving 27 per cent. as nitrogen, independently of that already considered as atmospheric air.

Of this gas 11 measures, containing 0·5 of oxygen, were fired with 28 of oxygen gas, which contained 26·9 of real oxygen.

Loss of volume due to formation of water...	=	10·5
Carbonic acid gas generated	=	5·5
Residual oxygen	=	16·4
Deducting 5·5 + 16·4 = 21·9 from 27·4 there remain, as Oxygen gas which went to the formation of water	=	5·5

A gaseous mixture, consisting of 50 measures of real marsh gas, 23 of air, and 27 of nitrogen, should have a specific gravity of 0·7724, since 0·5595 + 0·5 + 0·23 + 0·9727 + 0·27 = 0·7724.

The first of the foregoing analyses supplies an instance where the loss of carbon was decisive. In the second and third, as in the whole series of successful analyses, the carbonic acid gas may be taken as exactly equal to half the condensation due to the formation of water, and as containing half the oxygen which was required for complete combustion. The quantity of marsh gas present was equal to half the oxygen required for its complete combustion, to half the condensation due to generated water, and to the volume of car-

bonic acid gas which was produced. As this was a uniform result in all the samples, it is manifest that the constitution of the inflammable principle of fire-damp is identical with that of marsh gas or light carburetted hydrogen. The proportions of carbon and hydrogen indicated by analysis, sufficiently demonstrate the absence of such gases as hydrogen, carbonic oxide, and olefiant gas. Their absence, however, was proved by other methods. A portion of fire-damp was mixed in a tube with chlorine of known purity, and the mixture kept for a quarter of an hour in a dark place, when the chlorine was absorbed by milk of lime; the original quantity of fire-damp was always recovered, except a slight loss due to the mere washing to absorb the chlorine. The absence of olefiant and carbonic oxide gases was also proved by means of spongy platinum. In 1824, soon after the curious action of spongy platinum in causing the combination of oxygen and hydrogen gases was made known by Dœbereiner, both Dr. Henry and myself pointed out the obstacles to that action, occasioned by carbonic oxide, olefiant gas, and some other gases*. (Philosophical Transactions, and Edinburgh Philosophical Journal for 1824.) And Dr. Henry at the same time showed that marsh gas differs remarkably in this respect from carbonic oxide and olefiant gases, as it offers scarcely any impediment to the action of platinum. Agreeably to those researches, it follows that, if fire-damp contained merely marsh gas, oxygen, and nitrogen, spongy platinum introduced at common temperatures, or even heated to 300° Fahr., would not produce any sensible effect; and that if a small quantity of an explosive mixture † made with one measure of oxygen, and two measures of hydrogen gases, were added to the fire-damp, spongy platinum should cause a production of water corresponding to the quantity of explosive mixture so introduced, without the production of any carbonic acid. But if carbonic oxide or olefiant gas were present, then cold spongy platinum would not act at all, a small proportion of explosive mixture being employed; and if the action were forced by using hot spongy platinum, or by the free introduction of explosive mixture, then would carbonic acid as well as water be generated.

To apply these facts to the case in point, some very active platinum balls, of the size of peas, were made from a mixture of pipe-clay, spongy platinum, and the yellow ammoniacal chloride of platinum, the materials being mixed with water so as

[* Dr. Henry's paper on this subject, from the Philosophical Transactions, will be found in Phil. Mag. First Series, vol. lxv. p. 269.—Edit.]

† By the expression "explosive mixture," I hereafter mean a mixture made with one measure of oxygen and two measures of hydrogen gases.

to form a plastic mass, which, after receiving the required size and form, was gently dried, and ignited for an instant before the blow-pipe *, and were introduced into the gaseous mixture over mercury, sometimes cold and at others warm, ten or twenty seconds after incandescence. Their action on all the samples of fire-damp was precisely of the same character with fire-damp, oxygen being previously added or not; the platinum balls, whether cold or warm, were completely inactive. On adding some explosive mixture to the fire-damp, the platinum balls acted readily to their full extent. To give some instances.—

I. *With fire-damp from the yard coal seam, Burraton Colliery, the specific gravity of which was* 0·600.

With 46·5 measures of this gas, and 12·5 of explosive mixture, a platinum ball, nearly cold, caused in ten minutes a loss of volume equal to 12 measures.

In a second trial the loss in ten minutes was 13·6 in a mixture of 49 measures of fire-damp, and 14·1 of explosive mixture.

II. *With fire-damp from the Bensham coal seam, Wallsend Colliery, the specific gravity of which was* 0·6024.

In a mixture made with 34·3 measures of fire-damp, and 13·1 of explosive mixture, a platinum ball introduced warm, caused in six minutes a loss of volume equal to 12·4 measures.

With 43·5 measures of the same gas, and 22·9 of explosive mixture, the loss in eight minutes was 21·7, the platinum ball being introduced warm.

With 55 measures of the same gas, and 7 of explosive mixture, a cold platinum ball caused a loss of 6·3 in six minutes.

The action was equally rapid with the other gases; nearly the whole explosive mixture disappearing within the first or second minutes after the introduction of a platinum ball, whether warm or cold. In no instance did barytic water, subsequently admitted, detect in the residue a trace of carbonic acid gas.

When to any specimen of fire-damp hydrogen was added, the action of platinum always revealed the presence of air. When the quantity of air was small, the action of platinum was of course slow; nor did it in that case indicate with fidelity the quantity of air present, a portion of oxygen not uniting with hydrogen. Thus in the fire-damp from the yard coal seam, Burraton Colliery, nitrous gas indicated the presence of 6·2 per cent. of air, and platinum only 3·3 per cent. In the gas from the Bensham Seam, Wallsend Colliery, ni-

* Before use the little balls were always ignited.

trous gas indicated the presence of 9 per cent. of air; whereas platinum detected only 5 per cent. in one trial, 8·5 per cent. in a second, and 6 per cent. in a third. A certain degree of impediment to the action of platinum by marsh gas is thus rendered apparent. But when the fire-damp was freely mixed with air, then after the hydrogen gas platinum acted freely; and I have found under such circumstances the indications from platinum to coincide with those from nitrous gas. Thus in fire-damp from the low-main coal seam, Killingworth Colliery, of specific gravity 0·8226, platinum and hydrogen indicated 9·4 per cent. of oxygen, equivalent to 46·5 of air; and in two experiments with nitrous gas precisely the same result was obtained. A ball of platinum may hence be applied to determine the air in fire-damp, even when its quantity is small, by first diluting the gas with a known quantity of air, or enlivening the action of the platinum by adding some explosive mixture.

To those chemists who chance to be practically conversant with the action of platinum on gaseous mixtures, the evidence above adduced as to the freedom of fire-damp from hydrogen, carbonic oxide, olefiant gas, sulphuretted hydrogen, and similar inflammable gases, will, I doubt not, be quite satisfactory. To myself they do not leave the shadow of a doubt on the question. Those who are not familiar with such researches, may be warned that, in repeating my experiments, they will certainly fail of witnessing the same phænomena, unless they are very scrupulous in having pure gases, and in employing platinum balls with their full energy. The influence of platinum on gases is modified by such very slight circumstances, that a small matter will cause a ball to be wholly inert which would otherwise have acted with effect.

In applying nitrous gas to determine the quantity of oxygen in fire-damp, I employed the method of Dr. Dalton, as described in Dr. Henry's Elements of Chemistry. A measured quantity of fire-damp was added to the nitrous gas contained in a graduated tube half an inch wide, and the gases were allowed to act on each other over water, without agitation. The diminution of volume had attained its maximum in five or six minutes, and in general much sooner. Of the total loss, $\frac{10}{27}$ths were taken as oxygen. This method is not in all cases rigidly correct, but its indications were sufficiently exact for my purpose, controlled as they were by the action of platinum, by the analysis of the gas by detonation with oxygen, and by the specific gravity of the gases. Before relying at all on this method, however, I applied it in the analysis of gaseous mixtures containing known quantities of oxygen gas. On ap-

plying it to the analysis of atmospheric air it indicated 20·4 per cent. of oxygen. On agitating the air and nitrous gas, just after admitting them into the same tube, the diminution in volume was excessive. In a specimen of nitrogen gas, to which so much air was admitted that the whole mixture contained 3 per cent. of oxygen, nitrous gas indicated 3·3 per cent. of oxygen in one experiment, and 3·2 in a second. With nitrogen, which contained 3·6 per cent. of oxygen, nitrous gas indicated 4·4 in one trial, and in a second 4·1 per cent of oxygen. In nitrogen gas, with 4·7 per cent. of oxygen, nitrous gas indicated 4·7 per cent. in one trial, and 5·2 in a second. In nitrogen containing 7·3 per cent. of oxygen gas, nitrous gas indicated 7·4 in the first experiment, and 8·4 in the second. In the last case a large excess of nitrous gas was employed. In nitrogen gas in one experiment, and 11·5 in a second*. In this last case also nitrous gas was used in large excess.

	Mines in which the Gas was collected.	Specific Gravity. Observed	Specific Gravity. Calcul.	Marsh Gas.	Air.	Nitrogen.	Carb. acid.
1.	Bensham Coal Seam, Wallsend Colliery	0·6024	0·5991	91	9	0	0
2.	Yard Coal Seam, Burraton Colliery	0·600	0·5903	93	7	0	0
3.	High Main Seam, Killingworth Colliery	0·6196	0·6236	85	8	7	0
4.	Low Main Seam, Killingworth Colliery	0·8226	0·8325	37	46·5	16·5	0
5.	Marquis of Londonderry's Pensher Colliery, from the Hutton Seam Waste, 125 fathoms deep	0·966	0·9662	7	82	11	0
6.	Marquis of Londonderry's Pittington Colliery, Adelaide Pit, Hutton Seam, 45 fathoms below the surface	0·866	0·8755	28	67·5	4·5	0
7.	Eppleton Jane Pit, Hutton Seam, Hetton Colliery, 175 fathoms below the surface	0·747	0·7677	50	6	44	0
8.	Blossom Pit Main Coal Seam, Hetton Colliery, 100 fathoms below the surface	0·78	0·7724	50	23	27	0
9.	Bensham Coal Seam, Jarrow Colliery	0·6381	0·641	81·5	18·5	0	0
10.	Jarrow Colliery Seam, 11 fathoms below No. 9.	0·6209	0·6079	89	11	0	0
11.	Bensham Seam, Willington Colliery, 145 fathoms from the surface	0·7278	0·7175	68	28·7	0	3·3
12.		1·	1	0	100	0	

In these experiments the error is very uniformly such, that more oxygen was indicated than was actually present. The causes of error appear to be especially twofold,—agitation, and a large excess of nitrous gas. By permitting the action to ensue tranquilly, and avoiding much excess from nitrous gas, the indications in my trials were uniform, and very nearly true. Applying the same method to fire-damp, I found that

[* There appears to be some omission here. Edit. Phil. Mag.]

in two or more trials with the same gas the indications hardly ever differed so much as 1 per cent. of oxygen; and in general, as in several instances already given, the coincidence in different experiments was exact. Having now mentioned all that appears necessary to elucidate the chemical nature of the different samples of fire-damp from the mines of Newcastle, I conclude this account of the examination by inserting a tabular view of the composition of all the gases which have been analysed. [See Table in preceding page.]

The gas, No. 12, proved to be unmixed air. I have no remarks to offer respecting the nitrogen found in some samples of the fire-damp beyond what will readily occur to other chemists, who, I apprehend, will consider its presence as a simple consequence of oxidizing processes, especially of metallic sulphurets, abstracting oxygen from atmospheric air.

II. *Meteorological Observations during a Residence in Colombia between the Years* 1820 *and* 1830. *By* Colonel Richard Wright, *Governor of the Province of Loxa, Confidential Agent of the Republic of the Equator, &c. &c.*

IF the materials of science could be gathered only by the scientific, the following collection of observations would be a useless labour; but it frequently happens that, in distant countries, the opportunity of observing natural phænomena falls to the lot of those very ill fitted in most respects to profit by it. The genius of a Humboldt, like an incantation of science, descends upon the New World but once in a series of ages. The most that can be done by an ordinary observer is to offer his mite,—a single stone towards the pyramid of knowledge,—in the hope that he may casually prove useful; and with such humble pretensions can scarcely be deemed importunate. Should even this apology barely extenuate the sterility of a ten years' residence in a country so admirably varied and rich in natural phænomena as Colombia, something further may be urged in excuse of the *military* traveller, obliged frequently to *hurry* through the most interesting parts, and to vegetate whole years in others of minor importance; without books, without instruments, without resources; fettered too often by the chain of his own daily wants and sufferings; and fallen on a time when every species of local and traditional information, every glimmering of philosophic research had been buried and obliterated amid the storms and struggles of the revolution.

The geographical features of Colombia have been por-

trayed by Humboldt with an accuracy which renders further description superfluous. It is, however, impossible to traverse this extensive territory without being struck by the physical phænomena of a country where *height* produces the effect of *latitude*, and where the changes of climate, with all the consequent revolutions of animal and vegetable life, are brought about by localities to which we find little analogy in Europe. The equatorial seasons, as is well known, are merely the wet and dry; and though the Spaniards, influenced by European recollections, have given the former the name of winter *invierno*, it is during this period that nature revives from the vegetative torpor which the scorching tropical heats produce in the low-lands in almost an equal degree with the frosts of northern climates. In the vast plains which extend to the south and east of the great chain of the Andes the rainy season observes an invariable order. The Orinoco begins to rise in April, and attains its maximum of increase in July and August, when the immense savanas which extend to the base of the Andes are converted into the appearance of an inland ocean. It decreases from this period, and the summer is reckoned from October to April. In the mountains, on the contrary, the rains commence about the former month, and predominate, with intervals of fair weather, till May or June. The winter of the low lands, to the west and north of the Cordillera, both on the Pacific and Atlantic coasts, is governed by that of the mountains, but with several curious localities. Thus, the rainy season of Guayaquil is nearly as regular as that of the plains, being reckoned from the middle of December to the middle of May; while the thick forests, which further to the north cover the provinces of Esmeraldas, Barbacoas, and Choco, produce, by their constant evaporation, an almost perpetual deluge. Wherever, on the contrary, the Cordillera recedes to some distance from the coast, as is the case with parts of the Venezuelan chain, the intermediate country is parched by a drought often of several years. Maracaybo, and a considerable part of the province of Coro, are instances where sandy plains, scantily shaded by *Mimosas* and thick plants, afford shelter and subsistence only to flocks of goats and asses. The coast of Rio Hacha is equally dry and sterile, till it approaches the foot of the isolated ridge of Santa Marta; while the Goagira territory, situated betwixt Rio Hacha and Maracaybo, is regularly inundated every year, and consequently, though destitute of streams, maintains considerable herds of cattle and horses; a circumstance to be ascribed to the vicinity of the Ocaña branch of the Andes, which extends, with its clouds and thick forests, almost to

the confines of this province. The whole Peruvian coast from Payta to Lima is an additional instance of the same fact, where the recession of the Andes from the coast is marked by sandy deserts which the industry of the Incas had rendered productive by artificial irrigation. In the valleys and on the tablelands of the mountains themselves the culminating summits produce great variations in the distribution of moisture. The city of Caraccas, situated at the foot of the Silla, has the benefit of a regular though mild rainy season, while within a league there are spots which suffer several years of drought. Popayan, placed at the head of the sultry valley of the Cauca, and surrounded by lofty *paramos*, has nine months of continued rains and tempests, attributable to the clouds which are driven in opposite directions from the mountains till they encounter the hot ascending air of the valley. In the ancient kingdom of Quito, now called the Republic of the Equator, the mass of Chimborazo interrupts the passage of the clouds from south to north; so that, while the western slopes are deluged with rain, the elevated plains of Riobamba to the east recall to the imagination of the traveller the deserts of Arabia Petræa. Following the same mountain chain towards the city of Quito, we observe the storms arrested betwixt Cotopaxi and Pichincha, over the valley of Chillo; while two leagues further to the north the climate of the village of Pomasqui is so dry as to have given it the name of Piurita (little Piura).

The manner in which rain is formed and precipitated at various elevations, seems to illustrate and confirm the theory of Leslie. In the region of *paramos*, i.e. from 12,000 feet upwards, the encountering aërial currents, unless in the case of some strong agitation of the mass of surrounding atmosphere, are of a low and nearly equal temperature. The rains in consequence assume the form of thick drizzling mists, known by the name of *paramitos.* On the elevated plains we find the showers more or less sudden and violent, according to localities which give rise to a mixture of currents more or less variably heated. Quito, for example, is situated on what may be called a *ledge* of the lofty mountain of Pichincha, and overlooks the valley of Chillo or Guaillapamba, furrowing the adjacent table land, on which the thermometer often rises to 80° in the shade. The encounter of portions of the atmosphere, thus variously heated, produces showers as sudden and heavy as those which generally distinguish tropical climates. On the slopes of the Cordillera the rains are generally violent for the same reason. Looking to the hygrometrical state of the atmosphere, as it results from observations made

on the table lands of the equator and the coast of the Pacific, we find it to vary from 0° in the damp forests of Esmeraldas to 97°·1 on the elevated plain of Cayambe; the experiments in both places being made during June and July, the summer months both of the coast and mountains. The average medium for the low lands is 23°·85; for the Cordillera 44°·36 of the hygrometer constructed upon Leslie's principle; but we are in want of sufficient *data* for those elevations which approach to the limit of perpetual snow. To judge, however, from a small number of observations made on the mountain of Cayambe at 12,705 and 14,217 feet of elevation, and at the hut of Antisana at 14,520 feet, where the hygrometer was found to give 16°·5, 13°·9, and 30°·3, it would not seem that the dryness of the atmosphere increases in ratio of the elevation; at least in the neighbourhood of snowy mountains, where a continual moisture is exhaled, and heavy mists sweep over the soil towards evenings even of the fairest days.

To estimate the general distribution of temperatures through the vast territory of Colombia, we may conveniently consider it as divided into five zones. 1st, That of the level, or nearly so, of the ocean. 2nd, That of the small elevations, from 500 to 1500 feet. 3rd, That of the slopes of the Cordillera, from 2000 to 7000 feet. 4th, That of the elevated plains, or table lands, from 8000 to 10,000 feet; and 5th, That of the *paramos*, from 11,000 feet to the limit of perpetual snow.

1. The degree of heat at or near the level of the ocean is modified by a variety of local circumstances, which may be ranged under the following heads: proximity of the sea; of great rivers or lakes; of lofty ridges of mountains; of extensive forests; of contiguous elevations which impede the circulation of air, and produce reflected heat. The various combinations of these circumstances may be considered as affording a rule of the increase or diminution of temperature. Thus, La Guayra, situated on a sandy beach backed by a perpendicular wall of rocks, has no counterpoise to the excess of heat but the sea breeze, and the remote influence of the ridge of the Silla, which nowhere reaches the limit of perpetual snow. Humboldt considers it in consequence as the hottest place on the shores of the New World (Personal Narrative, vol. iii. p. 386.), the mean annual temperature being 82°·6; yet the observations I made during some months' residence in Maracaybo give an annual mean of 84°·63. Nor is this surprising, when we consider the localities of both places. In Maracaybo the sun's rays are reflected from a barren sandy soil, scantily sprinkled with *Mimosas* and prickly plants. The mountain chains are too remote to have any influence on the

atmosphere, so that several years frequently pass without any regular fall of rain. The vicinity of the lake, no doubt, acts slightly as a refrigerant; but the city is built on the border of its outlet to the sea, where it is both narrowest and shallowest, and is consequently heated nearly to the temperature of the incumbent atmosphere. Add to this, the small sandy elevations to the north, which intercept the partial effect of the sea-breezes, so that they are scarcely felt, except in the months of December and January, when the thermometer sometimes sinks to 73°; yet the medium even of these two months is not less than 81°; while that of La Guayra from November to December at noon, is, according to Humboldt, 75°·8, and at night 70°·9. (Personal Narrative, vol. iii. p. 387.) Rio Hacha is situated on a sandy beach; the sea-breeze blows with such violence that boats can scarcely land between ten in the morning and four in the afternoon. These winds, however, sweeping over the hot plains of Coro and Maracaybo, have but a partial effect in lowering the temperature, the annual mean of which is 1°·98 less than that of Maracaybo. I never saw the thermometer lower than 75°, nor above 89°. In Santa Marta the average of the coolest months is 82°·25. The thermometer, however, never rose during my residence there above 87°. The soil is sandy, and the city is surrounded by bare rocky heights to the north and south, which counterpoise the cooling influence of the *Sierra nevada* (snowy mountains), from which it is but a few leagues distant. The temperature of Barranquilla, a village situated on the river Magdalena, about eighteen miles from its mouth, is nearly the same with that of Santa Marta; for if, on the one hand, the air is refreshed by the evaporation from a damp soil covered with luxuriant forests and the vicinity of a large river, on the other, it is beyond the reach of the sea-breeze, and the influence of the mountains which operate in Santa Marta. The annual mean is 82°·20. That of Cumana is, according to Humboldt, 81°. The breezes which sweep from the gulf of Paria over the wooded Brigantine chain probably contribute to lower the temperature.

We have thus, on a calculation of six points on the Atlantic coast of Colombia, a mean annual temperature of 82°·56 *. The shores of the Pacific, as far as the latitude of Payta, are subjected to other influences, being almost entirely covered by damp luxuriant forests; while the ocean itself is cooled, as Humboldt observes, by the winds which blow continually

* I have not included Cartagena, because the number of observations is perhaps too limited to draw a conclusion as to the yearly temperature. If we take them into the calculation, the annual mean would be 82°·86, which is probably too high.

from the south. This, however, is more perceptibly the case from latitude 8° to 13°, where the air is cooled to an average of 71°·8. (Humboldt *De Distributione Geog. Pl.* p. 92). Betwixt 9° N. lat. and 3° S. lat., if we may trust to observations made at the five points of Panamà, Esmeraldas, El Morro, the island of Punà, and Guayaquil, the annual mean is 80°·11, being 2°·45 less than the mean of the Atlantic coast. A notable difference also arises from the superior elevation of the Pacific chain of the Andes, and its more immediate vicinity to the coast, while the Venezuelan branch, with the exception of the Santa Marta ridge, is both lower and more inland. A curious exception to the general temperature of the Pacific coast may be found on passing Punta Galera and Cabo San Francisco (lat. 50′ N.) to the south. The sky is here almost perpetually clouded, and a drizzling rain falls through the greater part of the year. During a week I passed there I never saw the sun; and the average temperature was only 74°·14. This was the more striking, as along the coast, immediately to the north of Punta Galera, the weather was constantly dry and the sky clear. The miry state of the road across the point of the Cape of San Francisco indicates the line of separation betwixt two distinct climates. It will be seen by the map, that from P. Galera the coast, after running nearly due west, turns abruptly to the south.

2. On penetrating into the interior of the country, and examining the temperature of small elevations, we may take, as forming an aggregate specimen of the whole country: 1. The damp wooded valleys of the Orinoco and Magdalena; 2. The forests which border on the Pacific; and 3. The immense plains of Venezuela, alternately flooded and parched with excessive heat. Humboldt assigns to the valley of the Orinoco a mean temperature of 78°·2. The small number of observations I have made on that of the Magdalena would give a mean of nearly 83°, which I should scarcely think too high, considering the localities of the river, which, flowing from south to north, affords no channel to the sea-breezes. Its mass of water is also much less considerable than that of the Orinoco; while its numerous sinuosities, and the low ridges which border it in the upper part of its course, contribute to render the air stagnant and suffocating. The temperature of Honda, at 1200 feet of elevation, is as high as that of any part of the coast, except Maracaybo. The unbroken forests which extend from the roots of the Quitenian Andes to the shores of the Pacific have a much lower temperature, caused by the proximity of the snow-capped Cordillera, and the humidity which prevails throughout the year. Accurate observations

give an annual mean of 76°·78, or 1°·42 lower than the valley of the Orinoco, and 6°·22 lower than that of the Magdalena. The mean temperature of the plains of Venezuela is reckoned by Humboldt at 88·4 (*De Distributione Geog. Plant.* p. 92.); yet several reasons may induce the belief that this calculation is excessive. This illustrious traveller performed his journey during the summer season, when the atmosphere is heated by the reverberations from a parched and naked soil. Persons who have resided near the Apure, state the climate in rainy weather to be cool, and refreshed by a constant breeze. It is only on the coast of the Pacific that the rainy season is the period of the greatest heat, when the air is still, and undisturbed by those electric explosions so common on the mountains and in the interior. The observations I made at Varinas and San Carlos, towards the beginning of the winter season, give a mean of 81°; and averaging the dry season at 88°·4, we have a yearly mean of 84°·7, which is probably the extreme, or something beyond it. There is no doubt it is in the plains of the interior we find the greatest heat during the dry season. In the level country, called the valley of Upar, betwixt the mountain ridges of Santa Marta and Ocaña, I found the thermometer in the shade several times above 100°, and once as high as 108°. The average of nineteen observations made at different points of this district is 89°·9; but we must allow a considerable decrease during the months when the soil is covered with thick vegetation, and drenched by continual rains. As a general mean of the interior, at small elevations, we may take 80°·67, or nearly that of Cumanà.

3. The temperate mountain region lies nearly betwixt the elevations of 3000 and 7000 feet. Below this may be considered as a hot climate, such, for instance, as Valencia and the valleys of Aragua in Venezuela, the height of which is from 1500 to 2000 feet, and its mean temperature 78°, or 0°·24 above that of Guayaquil on the Pacific; but the soil, stripped by cultivation of its ancient forests, imbibes freely the solar-rays, which are besides reflected from the rocky elevations which everywhere surround the cultivated districts. The temperature of Caracas (elevation 2904 feet) was fixed by Humboldt in his Essay *De Distributione Geographica Plantarum*, p. 98, at 69°·6; but in his Personal Narrative, b. iv. c. xii. p. 460, he considers 17°·2 of Reaumur = 70°·40 of Fahrenheit, nearly as the true yearly mean. My own observations during a residence of some months give 71°·40. The preference would be certainly due to Humboldt's calculation, but for some collateral circumstances deserving attention. I heard it generally remarked in the city, that the seasons had grown *hotter* since

the earthquake of 1812. It would be difficult to explain how the temporary evolution of volcanic gases, supposing such to have taken place, could operate any permanent change on the surrounding atmosphere; yet other causes may have produced an effect falsely ascribed to the phænomenon most impressed on the imagination of the inhabitants. On looking over Humboldt's collection of observations for December and January, 1799, we find the thermometer seldom rise to 75°, and often sink to 59°; so that the mean of these months is about 68°. During the same months in 1821, the daily range was from 65° to 76°. I never observed it lower than 61°·5, and on one occasion, at 5 a.m., it stood at 61°·0. The mean of these two months is 70°·21, or 2°·21 higher than the estimate of Humboldt. The clearness and beauty of the sky, during almost the whole period of my residence, is also a circumstance opposed to Humboldt's "*cœlum sæpe nubibus grave quæ post solis occasum terræ appropinquant.*"—*De Distributione Geog. Plant.* p. 98. I remember but *once* to have seen a fog in the streets of the city. Future observations will show whether any change of climate has really taken place, or whether the differences observed be only such variations as may be frequently remarked in the same place betwixt one year and another. The mean of the whole temperate mountain region may be reckoned at 67°·80; that is, if we limit ourselves to the districts partially cultivated and inhabited. The declivities of the Andes, still covered with vast and humid forests, have probably their temperature proportionably lowered. Thus the village of Mindo, on the western declivity of Pichincha, embosomed in humid forests, at 3932 feet of elevation, has a medium temperature of 65°·5, the same with that of Popayan.

4. The elevated plains of the Andes, betwixt 8000 and 11,000 feet, on which were anciently united the most powerful and civilized indigenous nations beneath the dominion of the Zipas of Tunja and Bogotà and the Incas of Quito, and where the great mass of Indian population is still to be found, have a general medium temperature of 59°·37, modified however by local circumstances, and particularly by the proximity of the *Nevados*. Thus the village of Guaranda, placed at the base of Chimborazo, though nearly 500 feet less elevated, is at least one degree colder than the city of Quito, sheltered on all sides by the ramifications of Pichincha. The city again is above one degree warmer than its suburbs on the plains of Añaguito and Turupamba to the north and south. Riobamba is about 200 feet below Quito; yet its situation on an open plain, bordered by the snowy mountains of Chimborazo, Tunguragua, and La Candelaria, renders the climate

colder and more variable; while the town of Hambato, only 300 feet lower than Quito, but built in a nook of the river which runs near it, and shut in by dry sandy elevations, has a climate about 2°·0 warmer; so that sugar-cane is cultivated in its immediate vicinity. The general uniformity of temperature, which spreads a certain monotony over tropical regions, is joined, at great elevations, to a daily variability which must exercise a considerable influence both on vegetable and animal life. The thermometer, which often sinks at night to 44°, rises, in the sun, wherever there is a reflected heat, frequently to 120°, being equal to the heat of Jamaica; while, in the shade, it seldom exceeds 65°; so that, on passing from shade to sunshine, one is immediately exposed to a difference of above 50°, and, in the course of twenty-four hours, to nearly 80°. The shade, in consequence, even on the hottest days, imparts a feeling of chilliness; while the solar rays seem to scorch like the vapour of a heated oven. The same difference is perceptible on the *paramos.* At the foot of the *Nevado* of Santa Marta I observed the thermometer at 5 a.m. sink to 22°; at 9 a.m. it rose to 73° in the sun. On the height of Pichan, betwixt Quito and Esmeraldas, elevation 12,986 feet, the thermometer stood at 53° in the shade, and 83° in the sun. On Antisana, the difference was 22° *at the same time*, but 34° betwixt 6 a.m. and 3 p.m. When the atmosphere is calm it is much more considerable.

5. Although at great elevations, i. e. from 12,000 to 16,000 feet, it is difficult to form a series of meteorological observations, such is the yearly equality of the temperature, that a single day may be safely taken as a sample of the whole year; nay, more, a collection of observations made at similar heights, though in different places, will give a similar result to a series taken on the same spot. Thus in the following table there is little difference betwixt the result of eight observations made on seven different mountains, and the six made on that of Antisana.

1.	Paramo of Santa Marta....	15,000 ft.	22° 5½ A.M.
2.	Paramo of Cayambe	12,705	37°·6 ,,
3.	Paramo of El Altar	12,986	42°·8 ,,
4.	Mine of Condorasto	14,496	45°·0 12
5.	Volcano of Pichincha	15,705	46°·0 1 P.M.
6.	Mountain of Atacaso......	14,820	41°·0 ,,
7.	Nevado of Cayambe	14,217	43°·0 1½ ,,
8.	Paramo of Antisana	14,520	38°·58 6 observations.
	General mean	....	39°

[To be continued]

III. *On Analytic Crystals.* By H. F. Talbot, *Esq.**

[Illustrated by Plates I. II. & III.]

In the course of experiments which I made with my polarizing microscope, I discovered a class of crystals which produce phænomena of great beauty, but of a nature very different from any that have been before described; for which reason a new name is necessary for them, and I have proposed that of *Analytic Crystals*.

I have given a brief description of them in the Philosophical Transactions for 1837 (page 32)†. But as I am well aware of the truth of the Horatian maxim,

> "*Segniùs irritant animos demissa per aures*
> *Quàm quæ sunt oculis subjecta fidelibus* ......"

I have here placed before the eye of the reader some coloured figures in illustration of the subject.

The usual method of displaying colours in crystalline bodies, consists in placing them between two tourmalines, called the *polarizer* and the *analyser*.

Instead of tourmalines the reflexion from plates of glass may be used; but whatever form of instrument is employed, it is necessary to have *two* of them. Each of these must be capable of polarizing common light. If not, it will not serve the intended purpose. A plate of sulphate of lime, for instance, placed vertically to the eye, will not answer either as a *polarizer* or an *analyser*.

The colours displayed by crystals thus placed between two tourmalines are well known to be very complex and numerous. The greatest variety of tints is often seen in the field of view at once.

Now, the crystals to which my present paper refers differ from the ordinary ones chiefly in two remarkable particulars.

(1.) They display, in the arrangement which I shall afterwards describe, two colours only at any given time. These colours are complementary to each other, and consequently in the strongest possible contrast.

(2.) The polarizing plate is employed alone, or in conjunction with a plate of sulphate of lime. *And no analysing plate is required.*

From which it will readily appear how differently characterized these phænomena are from the ordinary appearances of polarized light, although equally beautiful as microscopic objects.

These results may be obtained with great facility in the following manner:

* Communicated by the Author.

[† See Lond. and Edinb. Phil. Mag., vol. ix. p. 288; vol. x. p. 218.]

Dissolve boracic acid in boiling water, and put a drop of the solution between two plates of glass. It will immediately crystallize in irregular forms (Plate I. upper figure.).

Place it on the stage of the polarizing microscope using the *polarizer* only, *and no analyser.*

It will then be seen that these crystals (which are floating in water) present themselves under two aspects, *according to the direction in which they lie.* Those which lie one way appear with all their outlines strongly defined and extremely dark. Those which lie the other way (or vertically to the first) appear on the contrary as faint as possible. In our plate it was necessary to represent them as tolerably distinct, but in fact it often happens that their faintness is so extreme that they cease to be visible. This circumstance affords an example of one of the most curious things in optical science: for if one of the crystals has been carefully placed in this position, and a person be then desired to look into the microscope, he will confidently assert that there is no object in the field of view. If then the polarization of the light be reversed by turning the polarizer round 90°, the crystal will appear to start into existence, and become not only as dark as those represented in the figure, but even darker, or perhaps entirely opake.

Without entering into the theory of the matter here, it appears to me to result from this, that, of the two refractive indices of boracic acid *thus prepared*, one is the same with the refractive index of water.

If now, the appearance of things being as is represented in the upper figure of Plate I., we introduce a lamina of sulphate of lime below the crystals, the appearance changes to that represented in the lower figure.

If we turn the polarizer 90° round, the red crystals become green, and *vice versâ.*

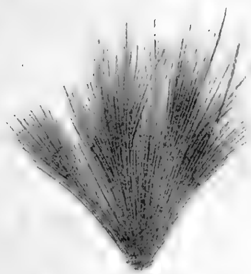
It is to be observed, that *the outlines only are coloured*; the central parts of the crystals are generally white.

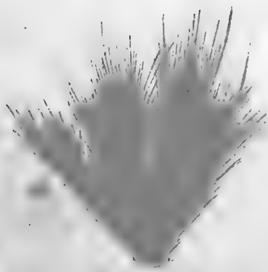
This coloration of the outlines constitutes the great beauty of some of these experiments. Its delicacy however is such as scarcely to admit of being successfully represented.

The upper figure in Plate II. represents another specimen of the same crystal, different in shape only. If now we exchange the lamina of sulphate of lime for one of different thickness, the colours change, as shown in the lower figure of the same plate.

Besides the boracic acid, I find the nitrates of potash and soda have this property in a high degree. No doubt the list may be considerably augmented; yet I think that the crystals

Pl 2.





which are destitute of this property are much more numerous than those which possess it.

I believe, that as a distinctive character this optical property may be of great service in chemistry. For instance, suppose that an exceedingly minute fragment of crystal (considered to be nitre) were under examination. Let it be viewed by polarized light in a manner analogous to what is above described; then if, when turned in various positions, it fails to develope two opposite colours, it follows that it cannot be a particle of nitre. I conceive that this mode of examination (when properly limited by experience) will prove a valuable auxiliary to the others already known.

Plate III. exhibits the phænomena produced by the capillary crystals of another salt—the oxalate of potash and chromium. The two figures represent the same object: the change of colour is effected by reversing the polarization of the light. It will be understood that the same change takes place by turning *the object* round 90°. From which it will be readily seen to follow, that if a complete circle were formed of these capillary crystals (all radiating from a point) two opposite quadrants of it would appear *green*, and the other two *red*. The crystallization of this salt is remarkably elegant, but I have been obliged to confine myself in the plate to the representation of one of its simplest forms, owing to the difficulty of doing justice to those which are more complicated. I have described it elsewhere*.

Concerning the theory of these very pretty phænomena, I need say nothing in the present place, because I think that I have given a satisfactory explanation, in the Philosophical Transactions, of the cause from whence they originate.

IV. *On certain Conditions under which Light is received from the Heavenly Bodies, and on the Importance of investigating them.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

I PRESUME upon your readiness to afford every facility to suggestions on scientific subjects, in sending you the few following remarks relating to a point connected with optical philosophy, which seems to deserve some consideration. Whether it really do contain anything worth further inquiry will be best ascertained by publicity among your readers.

* Phil. Trans. 1837, p. 33. [See L. & E. Phil. Mag., vol. x. p. 219.]

It has often struck me that there is a circumstance, apparently of moment, affecting the light imparted to us by the heavenly bodies, which is neglected by all inquirers, and of which, so far as I am aware, no sufficient notice has yet been taken. It is usual to reason as if the whole light supplied by each of those bodies were given out under the same identical conditions, as if from a mathematical point or centre; though a very little reflection must convince us that this is far from being either strictly, or even practically, the case; and the circumstance, therefore, to which I allude is the actual form and dimensions of those luminaries, conspiring to produce results, which, although they may be difficult at first sight to point out and explain, can scarcely be without their influence on some of the phænomena exhibited by that great natural agent.

It is obvious that, whatever theory be true with respect to the constitution of light, whether the Newtonian, the undulatory, or any other, the rays which reach us from a luminous body are propagated by a very complicated process from all parts of its surface; and although in straight lines always, yet in all directions outwards from each point in that surface. It is difficult for the mind to form any conception of the infinitely intricate system of radiation which is thus set in action. The instance so often given to illustrate the theory of undulations, of a stone dropped into a pool of water, is wholly inadequate to represent it. If a perfect sphere were allowed to fall into a fluid perfectly at rest, the wave caused by its displacement of the fluid would be impelled by a force acting simply in straight lines perpendicular to the surface of the sphere, in the plane of its horizontal great circle, which would be that of its maximum displacement; and therefore the force by which the wave would be driven upon any given point could only be exerted in a straight line joining that point and the centre of the sphere. If this were the case with the wave of light, the eye would see that point alone of the radiating surface through which such a line would pass; and as it does, in fact, receive the impression of light from every point upon it from whence a straight line can be drawn to the eye, it follows that the motion of light (whatever be its nature) is propagated by a system of infinite divergence at every conceivable angle from every portion of that surface.

If we now look at the bearing of these considerations upon the point to which we alluded at the outset, we must remark that the heavenly bodies are for the most part spheres, or spheroids, of enormous size; and under the small angle at which we view them we may be said to receive light from one

entire hemispherical surface at the same time. There must be some one spot in it so strictly and accurately opposite to the eye, that the straight line joining them, that is the course of the ray from thence, must be truly perpendicular to the surface of the hemisphere. But on every side of that point such lines must recede through all angular gradations, until at the utmost limits of the visible surface they become tangents to the sphere. In the course of this gradation, however, the rays are propagated, not merely at different angles, but at increasing distances; so that a ray leaving the circumference of the visible surface must have a considerably further space to traverse than one from its central point. We will apply ourselves to that body from whence we derive almost all our light, and consider for a moment what must be the result in the case of the sun. That luminary, whatever may be its substance, is a body of enormous size, having a diameter which, although the measurements may vary in a slight degree, must be somewhere about 800,000 miles, so that the difference of distance between that traversed by a ray from the nearest and from one of the extreme points of the visible hemisphere, must be about 400,000 miles, the length of its semidiameter. Now we know that light travels at the rate of about 192,000 miles in a second; and it follows that a ray from the circumference of the hemisphere must have received its impulse more than two seconds earlier than one from the superficial centre, in order that they may reach the eye at the same time.

It is difficult to suppose that so considerable a difference both in space and time should not exert a certain influence on some of the phænomena of light within our observation. In the first place, it is clear that the *aberration*, which is such as to be a matter of necessary correction in observations from this planet of the other heavenly bodies, must be greater by this difference in the one case than in the other; and the apparent place of the outer edge of the luminous hemisphere of any one of them must be erroneous in proportion, as compared with that of its superficial centre. In the next place, since it appears that, as light requires *time* for its propagation, there must be some actual *motion* involved in its passage from one point to another, be its constitution what it may, it is not unreasonable to suppose that the ultimate motion may be somewhat more feeble in the case of the greater distance; notwithstanding the extreme tenuity of its matter, which offers to bodies moving through it no sensible resistance. That light does lose strength as it becomes further removed from its source, is unquestionable; but this is for the most part an

effect due to diffusion and absorption. Yet as, in order to require time for its propagation, it must pass progressively from point to point throughout the whole course of its journey, it seems clear that it must consist of, or involve, matter in some form, however subtile and æthereal; and the analogy of all fluids would warrant the conclusion that there must be an actual loss of force when the moving power becomes more remote. If this be so, a ray of sun-light must consist of a series of rays between certain limits differing in their powers, and possibly in their properties; for when we consider the intimate dependence of the refraction of light, for instance, upon the density of the media through which it has to pass, and of its several forms of polarity upon the physical composition of the substances which produce it, who can say that these phænomena are beyond the reach of such a source of influence? We know so little of the mysteries of polarization, so little beyond its outward and visible effects, that it is impossible to conclude that it has no relation to such a circumstance; and although in both the above branches of optical inquiry I am well aware that much might be answered in respect of observations upon light from sources where no such cause of influence could exist, still so far as they have been made without a view to this particular point, they do not amount to a demonstration that in no case could any difference arising from it be detected.

There is another point which deserves consideration. Although the diameter of the sun subtends a sensible angle at the eye, still the distance is so great, and consequently that angle is so small, that we may assume the rays, even from its extremities, to be parallel, for the purpose of general reasoning. In that case it is evident that the series of increased distances to be traversed by them will be represented by the versed sines of the angles whereby they are removed from the centre of the visible hemispherical surface. Now if we suppose undulations or vibrations of any kind as the cause of the sensation of light, a supposition to which we are led by many optical facts, it is obvious, that while, in this series, there is a certain number of points which correspond exactly to those of the departure of successive waves, there must be also an intermediate number which would tend to neutralize the first, and to produce the well-known effect termed *interference*. Indeed, the series of points of propagation being unbroken over the whole curved surface, and mathematically continuous, it is difficult to conceive, upon the hypothesis of successive waves, how this effect can fail to take place, so immediately and absolutely, without an interval between one and another,

as to admit of the formation of a wave at all. It appears to constitute another difficulty in the way of that theory. According to the law of curvature, however, which must regulate the above series, there must of course be many more intervening rays between the points corresponding with the breadth of a wave in one part of the curve than in the other. There must be many more in that part where the versed sine increases least rapidly, near the superficial centre of the hemisphere, than where it increases most so, near its circumference; and therefore whatever effects may be due to such a cause must vary also according to the points from whence any given waves proceed. If any such interference does take place, is it not possible that it may have some connexion with those fixed dark lines which are so remarkable in the spectra of the sun, and of other heavenly luminaries*? The light of a lamp is asserted to display no such lines, though there is some inequality of brightness in its spectrum, which may be due to the unequal composition of the flame*. But if it appear that there must be some effects produced, and such as are likely to be in any degree sensible, there is room enough for speculation and inquiry.

I would notice one more point still. The rays proceeding from the extreme edge of a luminous hemisphere, and those near it, approximating almost infinitely to the position of a tangent, must, for some time after their first emission, pass at a very short distance from the surface. Now we know that, in the case of bodies within our own immediate observation, an effect takes place under certain circumstances termed *inflexion* or *diffraction*, whereby, without passing through any new medium, the rays of light are bent from their course by the mere passing near the surface of those bodies. One should suppose, therefore, that if such an effect is produced by so diminutive an object as those which have made it apparent to our observations, the immediate neighbourhood of so vast a mass of matter, for so long a time, as in the former case, must produce some also upon the course of the more remote rays, whether by decomposition or simple diversion. If the latter alone were to take place in any degree, this again would alter the apparent place of the circumference of the hemisphere by the amount of the deviation. If no such effect takes place at all, it is a question quite as well worthy of an answer, why it should be so? And wherein consists the cause of difference?

Indeed this last remark is one which may be applied to the

* Some observations on this subject by Sir J. F. W. Herschel will be found in L. & E. Phil. Mag., vol. iii. p. 406. See also vol. ix. p. 522.—Edit.

whole of the preceding observations. I have put them together for the purpose of suggesting to other inquirers certain sources of agency which it would appear must be necessarily followed by certain effects. That these should become sensible in themselves is what one should naturally expect; but if it should prove otherwise, it is worth while to inquire how it happens that they are lost. Whether, or how, they can be further traced I cannot now inquire; but it seems reasonable to suppose that considerations involving time and space to the amount of two seconds, and 400,000 miles, ought not to be neglected as indifferent to investigations, where appreciable phænomena supply measurements, in the case of a wave of light, to the *ten millionth* part of an inch, and the *quadrillionth* of a second.

I remain, Gentlemen, yours, &c.

November, 1838. J. S. W.

V. *On a remarkable Heat observed in Masses of Brine kept for some time in large Reservoirs. By* G. A. Prinsep, *Esq.**

IN the course of my experiments of several years in the manufacture of salt at Balya Ghát, on the salt-water lake east of Calcutta, I have sometimes observed a high degree of temperature at the bottom of the brine reservoirs after they had been filled for some weeks with brine of less than one fourth saturation. But as the greatest heat observed did not exceed 104° Fahr. which was under the maximum heat of the brine on the terraces, whence the reservoirs had been filled, I supposed the high temperature to be merely that of a warm stream of water let in at the hottest part of the day in May or June, and remaining below and unmixed with the cooler surface water, of less specific gravity, afterwards admitted. This opinion was strengthened by the gradual reduction of the temperature below to nearly that of the surface, before the end of the rainy season. I have frequently bathed in one of the reservoirs (about 550 feet long, 35 feet wide at top and 7 or 8 feet deep), in September and October, and have found the temperature of the water then pretty equal throughout. But on plunging into the same reservoir on the 17th September last, I was surprised to find the temperature near the bottom so warm as to be intolerable to the feet. Still however I imagined that the heat was only that which the sun had imparted to the terrace brine in the very sultry weather of June last, (1837) when I had 120° registered (4th June, 4 p. m.) for

* From the Journal of the Royal Asiatic Society of Bengal, vol. vii. p. 207.

the brine of a terrace yielding salt: and believing the hottest water to be therefore near the bottom I tried the temperature there about a month afterwards by immersing an empty bottle at the end of a bamboo, fixing the mouth so that it would be filled about a foot from the ground. The contents when poured out were at the temperature of 120°. A similar experiment made on the same day in a circular brine reservoir at Narainpore (120 feet diam. and about 16 feet deep) gave 104°. But on a subsequent visit to Narainpore on the 29th October, I was startled to observe that a pump fixed against the wall of this reservoir, for the purpose of feeding the boilers, was actually bringing up water of the temperature of 130° from a depth of about 12 feet. This very unexpected discovery determined me to contrive an instrument that should serve as a probe to ascertain both the temperature and the specific gravity or saltness of the water at different depths. Annexed is a drawing of the instrument employed: it consisted of a split bamboo with bamboo buckets fixed between at distances of one foot from centre to centre, the mouths of the buckets being corked, but the corks having small air-holes; and the mode of using the machine was, to let it down with the mouths of the buckets downwards, and then turn it round, after which the air bubbles indicated the progress of filling; and in ten minutes or a quarter of an hour, when these disappeared, the machine was quickly drawn up and the temperature of the water in the buckets was tried rapidly in succession with a small thermometer, leaving the specific gravity to be tried afterwards.

On the day of the first trial of this probe I was favoured with the company and assistance of Dr. Huffnagle, who took a lively interest in the experiment. The following particulars are the results of all the trials I have yet made with it, the buckets being numbered from the bottom of the machine.

First Experiment, 5th November, 9 A.M.

Open long reservoir at Balya Ghát. Probe immersed at an angle of 45° or 50°.

No.	Temp.					
1	106	only $\frac{1}{4}$ full.				
2	120	S.G.	(appt.)	1077	at T.	117
3	$120\frac{1}{2}$	,,	,,	1073·5	,,	$116\frac{1}{2}$
4	113	,,	,,	1071	,,	110
5	99	,,	,,	1049	,,	97
6	80	,,	,,	1022	,,	80
7	$78\frac{1}{2}$	,,	,,	1022	,,	78
8	78	,,	,,	1021	,,	78
9	78	,,	,,	1023·5	,,	78

Second Experiment, 5th November, 2 P.M. at Narainpore.

Open round brine reservoir. Probe at angle about 60° south-west side.

No.	Temp.					
1	105	(appt.)	S. G.	1163	at T.	100
2	104		not full,			
3	106	(appt.)	S. G.	1140	,,	104
4	113	,,	,,	1160	,,	108
5	117	,,	,,	1161	,,	113
6	123	,,	,,	1157	,,	117
7	130	,,	,,	1159	,,	123
8	132	,,	,,	1153·5	,,	124
9	137	,,	,,	1145	,,	130
10	131	,,	,,	1121	,,	125
11	127	,,	,,	1100	,,	120
12	122	,,	,,	1090	,,	114
13	114	,,	,,	1075	,,	109
14	104	,,	,,	1065	,,	101
15	100	,,	,,	1065	,,	97
16	85	,,	,,	1040	,,	84
17	84	,,	,,	1044·3	,,	83
18	82	,,	,,	not full.		
19	82	,,	,,	1038	,,	81

Third Experiment, 5th November, $2\frac{1}{2}$ P.M.

Same place and reservoir east side at gate. Probe at angle about 75°.

No.	Temp.					
1	102	(appt.)	S. G.	1149	at T.	100
2	106	,,	,,	1145·3	,,	103
3	109	,,	not full			
4	114	,,	S. G.	1175	,,	111
5	119	,,	,,	1165·5	,,	116
6	128	,,	,,	1159	,,	124
7	137	,,	,,	1155	,,	130
8	133	,,	,,	1139	,,	128
9	135	,,	,,	1125	,,	127
10	127	,,	,,	1097	,,	120
11	114	,,	,,	1075	,,	109
12	105	,,	,,	1068	,,	101
13	92	,,	,,	1050	,,	90
14	86	,,	,,	1040	,,	84
15	$82\frac{1}{2}$	,,	,,	1038	,,	81
16	$81\frac{1}{2}$	,,	,,	$1037\frac{1}{2}$	,,	81

Fourth Experiment, 19th November, 2 P.M., at Narainpore.

Open round brine reservoir south-west side. Probe at angle 60°.

No.	Temp.					
1	104	(appt.)	S. G.	1150	at T.	102
2	108	,,	not full.			
3	$108\frac{1}{2}$	,,	S. G.	1150	,,	106
4	114	,,	,,	1148	,,	112

No.	Temp.					
5	125	(appt.)	S.G.	1166	at T.	120
6	132	,,	,,	1151	,,	124
7	136	,,	,,	1142	,,	127
8	133	,,	,,	1126	,,	128
9	127	,,	,,	1095	,,	120
10	124	,,	,,	1070	,,	110
11	117	,,	,,	1061	,,	104
12	99	,,	,,	1057	,,	96
13	90	,,	,,	1047	,,	90
14	83	,,	,,	1046	,,	83
15	81½	,,	,,	1045·6	,,	83
16	81	,,	,,	1045	,,	83
17	82	,,	,,	1045	,,	83

Fifth Experiment, same date and place.

Covered reservoir. Probe at angle about 70°.

No.	Temp.			
1	88	(appt.)	S. G.	1147
2	88	,,	,,	1124·5
3	90	,,	,,	1107
4	91	,,	,,	1107
5	90	,,	,,	1102·6
6	90	,,	,,	1094
7	89	,,	,,	1081
8	88	,,	,,	1078
9	87½	,,	,,	1069
10	—	empty.		
11	82	,,	,,	1054
12	80	not full.		
13	77	not full.		
14	76			1046
15	76			1046

Sixth Experiment, same date and place.

Large reservoir. Probe at angle about 80°. Tried at 2½ P.M.

No.	Temp.			
1	93½	(appt.)	S. G.	1070
2	93½	,,	,,	1070
3	93	,,	,,	1069
4	92	,,	,,	1067
5	91½	,,	,,	1064
6	90	,,	,,	1064·5
7	87	,,	,,	1057
8	85	,,	,,	1056
9	84	,,	,,	1050
10	84	,,	,,	1050·5
11	84	(not full).		
12	84	,,	,,	1050

Seventh Experiment, 3rd December, 2 P.M. *at Narainpore.*

Open round reservoir, tried in the centre, probe nearly perpendicular.

No. 1	T. 107	half full.			
2	110	apparent S. G.	1151	at T.	106
3	114	,, ,,	1150	,,	110
4	118	,, ,,	1143·5	,,	118
5	125	half full.			
6	124	,, ,,	1114	,,	116
7	116	,, ,,	1095	,,	112
8	105	,, ,,	1078·5	,,	103
9	96	,, ,,	1063·5	,,	93
10	92	,, ,,	1059	,,	90
11	87	,, ,,	1054		
12	86	,, ,,	1053·7		
13	84	half full.			
14	82	,, ,,	1052		
15	81	,, ,,	1053		
16	82	,, ,,	1052		
17	82	,, ,,	1051		

In the first trial at Narainpore the greatest heat was found about half-way from the bottom. The difference in that respect at Balya Ghát, where the greatest heat appeared at the second and third foot from the bottom, may be explained by the small depth of the reservoir at the latter place, the surface water being liable to be affected to the same depth in both by the wind and rain and temperature of the atmosphere; and the subsequent descent of the maximum heat at Narainpore is attributable in part to the expenditure of the brine there, being pumped out from near the bottom for the supply of the boilers. The highest temperature given by the probe at Narainpore was 137°, but this is 5° less than the maximum given by the pumps, as will be seen by the following statement.

29 Oct.	N. pump T.	130	S. G. (cor.) 1180			
12 Nov.	,, ,, ,,	138	,, ,, 1170			
19 ,,	,, ,, ,,	142	,, ,, 1162			
26 ,,	,, ,, ,,	140½	,, ,, 1152			
3 Dec.	,, ,, ,,	137	,, ,, 1133	S. pump	134	S.G. 1172
10 ,,	,, ,, ,,	124	,, ,, 1173	,,	124	,, 1158
17 ,,	,, ,, ,,	125	,, ,, 1153	,,	124	,, 1175
24 ,,	,, ,, ,,	119	,, ,, 1173	,,	116	,, 1171
31 ,,	,, ,, ,,	116	,, ,, 1174	,,	114	,, 1179
7 Jan.	,, ,, ,,	102	,, ,, 1133	,,	106	,, 1128
13 ,,	(sunk 2 feet)	104	,, ,, 1177	,,	100	,, 1132
4 Feb.		90½	,, ,, 1100	,,	92	,, 1119
10 ,,	....	....	....	,,	90	,, 1110

As the temperature of 90° was only about the mean of June, and also that of the lower moiety of the brine in the

covered reservoir on the 19th November, which was all nearly of an equable temperature, I consider the influence of the heating course to have ceased in the first week of February, if not before. The reservoirs have since been pumped dry, and therefore these experiments cannot be repeated, until they are replenished with brine in April or May next.

It is remarkable that the probe indicated no signs of a heating influence affecting the water in the large reservoir at Narainpore on the 19th November, though the specific gravity of the brine near the bottom was little less than that of the water in the long reservoir at Balya Ghát on the 5th November, its mean spec. grav. being also considerably higher than the mean of the latter. Moreover the heating influence was scarcely traceable in the covered brine reservoir at Narainpore on the 19th November, which perhaps may be accounted for by the large previous expenditure of brine, say about three-fourths of its original contents, the consumption of which had been replaced to within a foot of the general level by filtration from the ground and leakage at the gate communicating with the adjoining terrace and brine fields; whereas the expenditure of brine in the contiguous open round reservoir otherwise similarly situated, was but half of the original contents up to the middle of January, its entire volume being about 170,000 cubic feet, while the covered reservoir contained only about 50,000. In these two reservoirs all the brine when first let in was of a high degree of saturation, ranging from 1170 to 1200 sp. gr. and consequently containing little or no sulphate of lime, which ingredient in the composition of sea-water, I have observed at Balya Ghát, is always deposited upon the terraces there, considerably before the brine begins to deposit its sulphate of soda. But this was not the case with respect to the brine in the large reservoir at Narainpore, nor in that of a longer narrow one at Balya Ghát, except perhaps a small proportion of the latter, both of which were charged with brine of only1070 to 1085 sp. gr., a much higher degree however than that of the contents of the long reservoir in any previous year; and in both of them the water had remained undisturbed, except by the action of the atmosphere; yet in one of them a high degree of heat was observed, and in the other, where I should sooner have expected to find it, no indication of heat was perceived beyond the probable temperature at which it was filled in June.

In order to ascertain however whether any fermentation and disengagement of heat takes place on the mixture of saturated brine with brine of a weaker degree, I lately procured from Balya Ghát some bottles of brine of different de-

grees of saturation, with which the following experiments were tried.

1*st Experiment.*—Half a pint of saturated brine sp. gr. 1216, temperature 82·5 mixed with about the same quantity of brine of sp. gr. 1069, temperature 81·2. Result, temperature 82·2 and no effervescence after standing some minutes.

2*nd Experiment.*—Same quantities of brine sp. gr. 1216, temperature 82·5, and of brine sp. gr. 1091, temperature 81°. Result, sp. gr. 1152·5, temperature 82·2 and no effervescence.

3*rd Experiment.*—Same quantities of brine sp. gr. 1216, temperature 82·5, and sp. gr. 1135, temperature 81·6. Result, sp. gr. 1174·3, temperature 82·1 and no effervescence, nor any increase of temperature after remaining some hours in the glass.

Being therefore quite unable to offer any explanation of the cause of the remarkable heat observed in my brine reservoirs, I can only promise to register the temperature from time to time when they are filled again, in the hope that materials may thus be furnished to some scientific friend more capable of solving the interesting problem. If it should be discovered that a slow fermentation arising from the mixture of brine of different densities in large masses is the cause of this heat, it would seem to be accelerated by agitation, for the water raised by the pumps was always warmer than that which the probe brought up from the same depth; and, except at the first trial at Narainpore, always hotter than the maximum given by the probe.

VI. *Notice of a Chemical Examination of a Specimen of Native Iron, from the East Bank of the Great Fish River, in South Africa. By* Sir John F. W. Herschel, *Bart., M.A., F.R.S.**

THE [portion analysed of the] specimen in question weighed originally 21·79 grains, 5·12 of which were separated, and submitted to a hasty preliminary examination for the detection of nickel, if any; but the quantity proving too small, the whole of the remainder was operated on in a subsequent trial.

The iron was highly malleable and tenacious, and apparently of excellent quality, with a somewhat whiter and more silvery lustre than belongs to the metal in its ordinary state,

* Read before the Literary and Scientific Institution of South Africa: now extracted from Sir James E. Alexander's "Expedition of Discovery into the Interior of Africa." Lond. 1838, vol. ii. Appendix, p. 272. The specimen had been found by Sir James Alexander, and presented by him to the Institution.

and apparently little liable to oxidation, qualities which are observed in iron, of what is usually considered undoubted meteoric origin.

I should not think it necessary to detail the steps of the analysis by which the presence of nickel in the proportion of 4·61 per cent. was demonstrated, but for a peculiarity in one part of the process by which an inconvenience of frequent occurrence in chemical operations, and of a very embarrassing nature, was obviated, and which may prove useful as a hint to young analysts in other cases.

18·67 grains of the iron in one piece were digested in dilute nitric acid, which dissolved the whole, with the exception of a trifling quantity of black scaly matter, apparently amounting to about a quarter of a grain*. Towards the end of the solution the iron more than once brightened on the surface, and assumed that peculiar and singular state of resistance to the action of the acid which I have described in the *Annales de Chimie* for September 1833, and which has since been the subject of so much interesting discussion by Professor Schœnbein, Mr. Faraday, and others†. In consequence, it was necessary to apply and maintain heat to complete the solution.

The nitric solution was evaporated to dryness, water added, and evaporated a second and a third time. By this the whole of the iron was peroxidized, and nearly the whole separated. It was then diffused and boiled in water, to which a few drops of nitric acid were added, to take up any oxide of nickel which might have been deprived of its acid by overheating, and set aside for subsidence, filtration being out of the question.

After standing a week, however, it was still perfectly opake, and loaded with suspended peroxide of iron, and to get rid of this was the next object.

Lead being a metal easily eliminated, and incapable of interfering in any of the subsequent processes, its introduction seemed not likely to prove any source of further embarrassment; a few drops of dilute nitrate of lead were therefore added; and being well mixed, as much sulphuric acid as would saturate the lead, and a little more, was added, and the whole boiled. The precipitation was complete, the lead carrying down with it all the suspended ferruginous matter, and leaving a clear liquid of a greenish hue, in which the presence of lead could not be detected.

* This black scaly matter was in all probability *graphite*.—Edit.

† See L. and E. Phil. Mag. vol. xi. p. 329.—Edit.

The remaining iron held in solution was removed by heating it with excess of carbonate of lime, in the manner pointed out by me in the Phil. Trans. for 1821*, when after filtration, a liquid remained of that peculiar tint of pale green which characterizes the solutions of nickel, and of considerable intensity.

The presence of this metal was ascertained on concentrating the solution by the usual tests, and its quantity concluded, viz. 0·86 grains, or 4·61 per cent. on the specimen analysed.

Thus it appears that the specimen brought home by Capt. Alexander has equal claim to a meteoric origin with any of those masses of native nickeliferous iron which have been found in different localities, and to which that origin has, without other evidence, been attributed.

All those specimens, however, have, so far as I know, been insulated single masses. But what constitutes the peculiar and important feature of this discovery of Capt. Alexander, is the fact stated by him of the occurrence of masses of this native iron in abundance, scattered over the surface of a considerable tract of country. If a meteoric origin be attributed to all these, a shower of iron must have fallen; and as we can imagine no cause for the explosion of a mass of iron, and can hardly conceive a force capable of rending into fragments a cold block of this very tenacious material, we must of necessity conclude it to have arrived in a state of fusion, and been scattered around by the assistance of the air or otherwise, in a melted, or at least softened state.

VII. *Supplementary Note to Experimental Researches in Electricity.—Eleventh Series. By* MICHAEL FARADAY, *Esq., D.C.L. F.R.S. Fullerian Prof. Chem. Royal Institution, Corr. Memb. Royal and Imp. Acadd. of Sciences, Paris, Petersburgh, Florence, Copenhagen, Berlin, &c. &c.*†

1307. I HAVE recently put into an experimental form that general statement of the question of *specific inductive capacity* which is given at No. 1252 of Series XI., and the result is such as to lead me to hope the Council will authorize its addition to the paper in the form of a supplementary note. Three circular brass plates, about five inches in diameter, were mounted side by side upon insulating pil-

* Sir John Herschel's paper here alluded to will be found in Phil. Mag. First Series, vol. lix. p. 86; and in the Annals of Philosophy, New Series, vol. iii. p. 95.—EDIT.

† From the Philosophical Transactions for 1838, Part i.

lars; the middle one, A, was a fixture, but the outer plates B and C were moveable on slides, so that all three could be brought with their sides almost into contact, or separated to any required distance. Two gold-leaves were suspended in a glass jar from insulated wires; one of the outer plates B was connected with one of the gold-leaves, and the other outer plate with the other leaf. The outer plates B and C were adjusted at the distance of an inch and a quarter from the middle plate A, and the gold-leaves were fixed at two inches apart; A was then slightly charged with electricity, and the plates B and C, with their gold-leaves, thrown out of insulation *at the same time*, and then left insulated. In this state of things A was charged positive inductrically, and B with C negative inducteously; the same dielectric, air, being in the two intervals, and the gold-leaves hanging, of course, parallel to each other in a relatively unelectrified state.

1308. A plate of shell-lac three quarters of an inch in thickness, and four inches square, suspended by clean white silk thread, was very carefully deprived of all charge (1203.), so that it produced no effect on the gold-leaves if A were uncharged, and then introduced between plates A and B; the electric relation of the three plates was immediately altered, and the gold-leaves attracted each other. On removing the shell-lac this attraction ceased; on introducing it between A and C it was renewed; on removing it the attraction again ceased; and the shell-lac when examined by a delicate Coulomb electrometer was still without charge.

1309. As A was positive, B and C were of course negative; but as the specific inductive capacity of shell-lac is about twice that of air (1270.), it was expected that when the lac was introduced between A and B, A would induce more towards B than towards C; that therefore B would become more negative than before towards A, and consequently, because of its insulated condition, be positive externally, as at its back or at the gold-leaves; whilst C would be less negative towards A, and therefore negative outwards or at the gold-leaves. This was found to be the case; for on whichever side of A the shell-lac was introduced the external plate at that side was positive, and the external plate on the other side negative towards each other, and also to uninsulated external bodies.

1310. On employing a plate of sulphur instead of shell-lac, the same results were obtained; consistent with the conclusions drawn regarding the high specific inductive capacity of that body already given (1276.).

1311. These effects of specific inductive capacity can be exalted in various ways, and it is this capability which makes the great value of the apparatus. Thus I introduced the shell-lac between A and B, and then for a moment connected B and C, uninsulated them, and finally left them in the insulated state; the gold-leaves were of course hanging parallel to each other. On removing the shell-lac the gold-leaves attracted each other; on introducing the shell-lac between A and C this attraction was *increased*, (as had been anticipated from theory,) and the leaves came together, though not more than four inches long, and hanging three inches apart.

1312. By simply bringing the gold-leaves nearer to each other I was able to show the difference of specific inductive capacity when only thin plates of shell-lac were used, the rest of the dielectric space being filled with air. By bringing B and C nearer to A another great increase of sensibility was made. By enlarging the size of the plates still further power was gained. By diminishing the extent of the wires, &c. connected with the gold-leaves, another improvement resulted. So that in fact the gold-leaves became, in this manner, as delicate a test of *specific inductive action* as they are, in Bennet's and Singer's electrometers, of ordinary electrical charge.

1313. It is evident that by making the three plates the sides of cells, with proper precautions as regards insulation, &c., this apparatus may be used in the examination of gases, with far more effect than the former apparatus (1187. 1290.), and may, perhaps, bring out differences which have as yet escaped me (1292. 1293.).

1314. It is also evident that two metal plates are quite sufficient to form the instrument; the state of the single inducteous plate when the dielectric is changed, being examined either by bringing a body excited in a known manner towards its gold-leaves, or, what I think will be better, employing a carrier ball in place of the leaf, and examining that ball by the Coulomb electrometer (1180.). The inductive and inducteous surfaces may even be balls; the latter being itself the carrier ball of the Coulomb electrometer (1181. 1229.).

1315. To increase the effect, a small condenser may be used with great advantage. Thus if, when two inducteous plates are used, a little condenser were put in the place of the gold-leaves, I have no doubt the three principal plates might be reduced to an inch or even half an inch in diameter. Even the gold-leaves act to each other for the time as the plates of a condenser. If only two plates were used, by the proper

application of the condenser the same reduction might take place. This expectation is fully justified by an effect already observed and described (1229.).

1316. In that case the application of the instrument to very extensive research is evident. Comparatively small masses of dielectrics could be examined, as diamonds and crystals. An expectation, that the specific inductive capacity of crystals will vary in different directions, according as the lines of inductive force (1304.) are parallel to, or in other positions in relation to the axes of the crystals, can be tested: I purpose that these and many other thoughts which arise respecting specific inductive action and the polarity of the particles of dielectric matter, shall be put to the proof as soon as I can find time.

1317. Hoping that this apparatus will form an instrument of considerable use, I beg to propose for it (at the suggestion of a friend) the name of *Differential Inductometer.*

Royal Institution, March 29, 1838.

VIII. *On the Equilibrium of Fluids, occasioned by an Article of Professor* Sylvester *on Fluids, published in this Journal for December* 1838. *By* J. Ivory, *K.H., F.R.S.**

PROFESSOR Sylvester has deduced the usual laws of the equilibrium of incompressible fluids from a principle proposed by the eminent mathematician and philosopher Gauss. Not to speak of the motion of fluids, the theory of which is pressed by so many difficulties, the results respecting equilibrium obtained by the Professor coincide entirely with what has been investigated from principles, seemingly different, by Clairaut, Euler, Lagrange, Laplace, and Poisson, this last having extended his views to the molecular forces that act upon the particles. Such a concurrence in the conclusions arrived at, begets a suspicion that in reality all the processes are guided by one invariable principle, which, although hidden under algebraic operations, necessarily conducts the analyst to the same landing-place. To prove that this is actually the case is the intention of what follows.

The general laws of equilibrium in question are deducible from the following theorem, which is grounded on the most general conception of fluidity that can be formed. *In a fluid at rest, the particles of which are urged by accelerating forces, the pressure at the several points of the mass, can be mathematically expressed only by a function of the coordinates that*

* Communicated by the Author.

determine the position of the point of action, these coordinates being considered as unrelated and independent quantities. To prove this, take any two points of the mass, and let a communication be made between them by a canal of any figure: if to the pressure of the fluid on the orifice of the canal at one end, we add the effort of the fluid contained in the canal, caused by all the forces that urge the particles in the direction of the canal, the result will counterbalance the pressure on the orifice at the other end; wherefore, in a fluid at rest, the effort of every canal, of whatever figure, will be the same, provided the extreme orifices are the same. Now it is easy to ascertain that this property will be fulfilled when the sort of function described in the theorem stands for the pressure; and an application of the method of variations will show that no other sort of function will answer the same end.

The theorem being demonstrated, let X, Y, Z denote the accelerating forces that urge a particle of the fluid in the respective directions of its coordinates, x, y, z; the differential of the pressure will be

$$X\,dx + Y\,dy + Z\,dz: \qquad \text{(a.)}$$

and, as the fluid is at rest, this must be the differential of a function of a certain kind, by the theorem; and, on the other hand, if it be an exact differential, it can be derived only from a function of the sort mentioned. Wherefore, in a fluid in equilibrium, the expression (a.) is always an exact differential, and it is zero at the upper surface, because the pressure is constant at that surface.

The foregoing reasoning holds, whatever modification, or assumption, is superadded to the notion of fluidity. The mathematical laws of the equilibrium of fluids are thus placed on the broadest foundation. They are sufficient for determining the figure of equilibrium when the integral of (a.) is an explicit function of the coordinates; but they are not sufficient when the same integral is only an implicit function, that is, when the forces in action are derived from different sources, and are independent on one another; in which case, as common sense dictates, recourse must be had to the peculiar circumstances of the problem in order to complete the solution.

It deserves to be noticed that the theorem is true, and the expression (a.) is an exact differential, in a fluid at rest, but not in one in a state of motion; which marks a distinction between the mathematical laws that govern the two cases: yet it is usual to deduce the motion of fluids from their equilibrium.

December 17, 1838. JAMES IVORY.

IX. *Observations on Shooting Stars made on the Night of November* 12 *to* 13, 1838, *at Whitechapel, London. By Mr.* W. R. Birt, *Librarian and Assistant Secretary to the Metropolitan Literary and Scientific Institution.**

D. Signifies that the motion of the shooting star was direct, R. that it was retrograde.

No. *Time.* *Direction.*

1. 9 20 From middle Ursa Major to Auriga. R.

2. 9 30 From between Gemini and Orion's head to horizon, inclined to north: D.—*Character*: Brilliant, slow.

3. 9 50 From θ Aurigæ to θ Geminorum. D.—Faint.

4. 10 20 From middle of Ursa Major to between θ Gem. and Castor. R.—Very faint.

5. 10 45 From Gemini across β Tauri to between the Pleiades and Hyades. R.—Very swift.

6. 10 47 From north of Capella to middle of Ursa Major towards the horizon. D.—Very brilliant. Train.

† 7. 10 55 From below Pollux between γ and ξ Gem. to α Orionis. R.—Very brilliant with splendid blue train.

? 8. 11 5 From about three degrees below Pollux towards the horizon. D.—Very short.

? 9. 11 27 From north of Capella to middle of Ursa Major. D.—Faint.

10. 11 30 From θ Aurigæ towards Ursa Major. D.—Small.

11. 11 44 From a little below β Canis Minoris towards E. inclined to horizon. D.—Small.

? 12. 11 50 Across β Canis Majoris towards the south. R.

13. 11 54 From Castor and Pollux across Aldebaran. R.—Very swift, slight train.

14. 11 59 Between Capella and β Aurigæ towards N. little E. of zenith. R.—Bright.

15. 12 17 Midway between Leo and Ursa Major towards north. D.—Small, long.

16. 12 22 Between the feet of Ursa Major towards Virgo. D.—Small.

17. 12 24 North of Leo towards the north. D.—Small.

18. 12 28 Between α and γ Leonis towards Virgo. D.—Small.

19. 12 34 From ϵ past γ Geminorum towards Sirius. D.—Small.

12 40 Bright light horizon NE. D.

* Communicated by the Author.

No. *Time.* *Direction.*

20. 12 45 From λ and μ Ursæ Majoris past ν and ξ U. M. to tail of Leo. D.—Small.

21. 12 54 Between β Canis Majoris and κ Orionis towards south. R.

22. 12 57 Across β Leporis to west. R.

23. 13 5 From Canis Minor to Cancer. D.—Small.

24. 13 13 From Cancer to Caput Hydræ. D.—Small.

25. 13 26 From Leo Minor to γ Leonis. R.—Small.

26. 13 28 From χ to η Ursæ Majoris. D.

27. 13 29 From γ to between ι and (λ and μ) Ursæ Majoris. R.

28. 13 37 From ι to γ Ursæ Majoris. D.

† 29. 13 54 From ν Ursæ Majoris to α Canes Venatici. D.—Brilliant. Train some seconds.

30. 14 3 From ϵ Ursæ Majoris past ζ by north of these stars. D.—Small.

31. 14 7 From Leo towards Procyon between Cancer and Caput Hydræ. R.—Small.

32. 14 14 From Caput Leonis across Cancer to β Canis Minoris. R.—Bright, very swift.

33. 14 17 From 22 Monocerotis to γ Geminorum. R. —Small.

34. 14 25 In Leo Minor towards the horizon. D.—Very small.

35. 14 26 From ι to γ Ursæ Majoris. D.—Small.

14 28 Splendid Aurora Borealis. Streamers. greenish, summits deep red, ι θ Draconis.

36. 14 36 From ν Ursæ Majoris between α Canes Venatici and β Leonis. D.

37. 14 44 To the north of Leo from μ to o. D.—Bright.

38. 15 1 Between Draco and tail of Ursæ Majoris towards horizon. D.—Small.

15 4 Second coloured Aurora.

39. 15 5 From θ to Cor Hydræ. D.

40. 15 5 Small one at right angles to above Caput Hydræ. R.

41. 15 21 From β and γ Ursæ Minoris to ι Draconis. D.—Irregular, partly extinguished.

42. 15 32 Between α Canes Venatici and Coma Berenices towards Bootes. D.

43. 15 44 From γ past ϵ to ζ Ursæ Majoris. D.—Swift.

44. 15 51 From ψ past ν Ursæ Majoris to o Leonis. D. —Bright.

16 0 Third coloured Aurora.

No. Time. Direction.

45. 16 3 Considerably below Draco between it and Bootes N.E. D.—Very brilliant.

46. 16 12 From between α Canes Venatici and Coma Berenices towards Bootes. D.—Swift? small train.

47. 16 18 From o past β and γ Draconis. R.—Path curved.

48. 16 24 From Leo Minor to Coma Berenices. D. —Small.

49. 16 43 From η Ursæ Majoris to Caput Draconis. D.

50. 16 59 From below the moon towards the horizon. D.—Bright.

51. 17 8 From between α Canes Venatici and Coma Berenices to Arcturus. D.—Bright, swift.

52. 17 11 Past η Bootes towards the horizon. D.—Small.

53. 17 14 From θ to β Bootes. D.—Small.

54. 17 25 From Coma Berenices to γ Bootes. D.—Small.

The †'s prefixed indicate the two most brilliant of the shooting stars observed.

I am not certain which of the two 45 and 46 left the small train; it was observed with one of them.

At the commencement of the observations the meteors appeared to emanate from, or approached to, a line drawn through Castor and Pollux; as the night advanced and Gemini approached the meridian, this line was frequently crossed; still a line parallel to it might be regarded as the axis which the meteors either emanated from or approached to, until they becoming more widely diffused over the eastern hemisphere, and their directions more varied, such an axis ceased to be recognised.

As the observer commanded only the eastern quarter of the heavens from north to south, an inspection of the following table, in which the meteors are arranged according to the constellations in or near which they appeared, will show where they were most numerous in that quarter, namely, a little north of east. The constellations in the second column were within view of the observer during the night, while they were above the horizon. The whole of the constellations in the table are arranged (as nearly as possible for this method) according to their relative situations in the heavens.

From Ursa Minor			In Ursa Major								
To Draco	41	D.	χ to η	26	D.						
From Draco			γ to μ	27	R.						
To N.E.	45	D.	ι to γ	28	D.						
In Draco			ε past ζ	30	D.						
$\beta\,\alpha\,\gamma$ to o	47	R.	ι to γ	35	D.						
			γ past ε and ζ	43	D.						
In Bootes			From Ursa Major						From Auriga		
Past η	52	D.	To Gemini	1	R.				To Gemini	3	D.
θ to β...	53	D.	,, ,,	4	R.				,, Ursa Maj.	6	D.
			,, Leo	20	D.				,, ,, ,,	9	D.
			,, ,,	44	D.				,, ,, ,,	10	D.
From Canes Venat.			,, Virgo	16	D.				,, North	14	R.
To Bootes	42	D.	,, Canes Venatici	29	D.						
,, ,,	46	D.	,, ,,	36	D.						
,, ,,	51	D.	,, Horizon	38	D.						
			,, Draco	49	D.						
			In Leo Minor	34	D.						
			From Leo Minor			From Gemini			From Orion		
			To Leo	25	R.	To Taurus	5	R.	To horizon	2	D.
From Coma Ber.			,, Coma Berenices	48	D.	,, ,,	13	R.			
To Bootes	54	D.	From Leo			,, Orion	7	R.			
			To Canis Minor	31	R.	,, Canis Major	19	D.			
			,, ,, Major	32	R.	,, Horizon	8	D.			
			,, Virgo	18	D.	From Canis Minor					
In Virgo			,, North	17	D.	To East	11	D.			
Below Moon	50	D.	,, ,,	15	D.	,, Cancer	23	D.			
			,, ,,	37	D.	From Monoceros					
			From Cancer.			To Gemini	33	R.			
			To Caput Hydræ	24	D.	From Canis Major			From Lepus.		
			In Hydra			To South	12	R.	To West	22	R.
			θ to Cor.	39	D.	,, ,,	21	R.			
			Right angles to	40	R.						

X. *On the Voltaic Polarization of certain Solid and Fluid Substances.* *By Prof.* Schœnbein.

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

I READ a paper containing an account of the results of my researches on the voltaic polarization of solid and fluid bodies, before the scientific meetings which took place at Bâle and Fribourg, some months ago.

This memoir, I think, will soon be published in the *Biblioth. Univ.* and in Poggendorff's *Annalen*, and the scientific public thereby enabled to appreciate the facts related in it. The sort of investigations alluded to could not but lead me to make numerous experiments similar to those which were mentioned the other day in the French Academy, as performed by MM. Matteucci * and Peltier, of which I have however, up to this present moment, but a very imperfect knowledge. Having from want of time not yet been able to draw up a regular paper on the results of my most recent voltaic researches, and thinking them not quite void of scientific interest, I beg the favour of your giving in the forthcoming Number of your widely circulated Journal a place to the general statements, the tenor of which is as follows:

1. A platina wire polarized either in the positive or negative way loses its peculiar condition by being heated red-hot. (I call positively polarized a wire which has acted for some time the part of the negative electrode in water slightly acidulated by sulphuric acid; and I term negatively polarized a wire which has in the same liquid performed the function of the positive electrode.)

2. A platina wire positively polarized loses its peculiar condition by being plunged only for a single moment into an atmosphere of chlorine.

3. A platina wire positively polarized loses likewise its electromotive power by being placed in an atmosphere of oxygen; but in order to destroy entirely the polarity of the wire, it is necessary that it should remain for some seconds in the gas mentioned.

4. A platina wire negatively polarized loses its peculiar condition by being put into an atmosphere of hydrogen, but in order to obtain this effect, it is required that the wire in question should remain for some seconds in the gas.

5. A platina wire polarized either negatively or positively is not sensibly affected by being placed in an atmosphere of

* See p. 469 of our last number and volume.—Edit.

carbonic acid or in one of any other gas which does not chemically act either upon hydrogen or oxygen.

6. A platina wire (in its natural state) assumes in every respect the condition and voltaic bearings of a positively polarized wire by being plunged only for a few seconds into an atmosphere of hydrogen.

7. Gold and silver are not sensibly affected under the same circumstances.

8. A platina wire does not acquire any degree of electromotive power by being put into oxygen gas: the metal remains in its natural state. Neither is any degree of such power acquired by gold or silver under the same circumstances.

9. Platina, gold, and silver, by being placed only for a few seconds in an atmosphere of chlorine, assume the voltaic state of a negatively polarized wire.

10. Water slightly acidulated with sulphuric acid and holding some hydrogen dissolved, bears to acidulated water containing no hydrogen the same voltaic relation that zinc does to copper; provided, however, both fluids be separated from each other by a membrane, and connected with the galvanometer by means of platina wires. If for the latter purpose (that is to say, for connecting the fluids with the galvanometer) gold or silver wires are made use of, the said fluids do not excite the least current.

11. Two fluids, one being acidulated water containing some oxygen dissolved, the other being likewise acidulated water containing no oxygen, appear to be in a voltaic point of view perfectly indifferent to each other, whether they are connected with the galvanometer by platina, silver, or gold wires.

12. Water slightly acidulated with sulphuric acid and holding some chlorine dissolved, bears to acidulated water not containing any chlorine the same voltaic relation that copper bears to zinc. In other terms, the former fluid acts under certain circumstances the electromotive part of the peroxides of silver, lead, &c.

13. The aqueous solution of hydrogen mentioned in § 10, loses its property to excite a current by being mixed with a certain quantity of an aqueous solution of chlorine; and, *vice versâ*, the latter fluid loses its electromotive power mentioned in the § 12 by being mixed with a sufficient quantity of hydrogen dissolved in water.

14. Muriatic acid positively polarized loses its peculiar voltaic condition by being mixed with some chlorine, and the same acid being negatively polarized loses its polarity by being treated with some hydrogen. From the facts stated, and

others which are mentioned in the memoir above alluded to, a great number of rather important inferences might be drawn; but having for the present no leisure time to do so, I am obliged to confine myself to stating those which follow:

a. The secondary currents produced both by polar wires, electrolytic fluids, and secondary piles, are due to chemical action, i. e. (in the cases mentioned) to the union of oxygen with hydrogen, or to that of chlorine with hydrogen; and not, as M. Peltier seems to think, to the mere act of the solution in water of the gases mentioned.

b. The chemical combination of oxygen and hydrogen in acidulated (or common) water is brought about by the presence of platina in the same manner as that metal determines the chemical union of gaseous oxygen and hydrogen.

c. The current produced by a platina wire being surrounded by a film of chlorine, or by water holding chlorine in solution, is not dependent on the action of the latter body upon platina, but on the action of chlorine upon the hydrogen of water.

d. Electrolytic bodies do not suffer even the weakest current to pass through them without undergoing decomposition. (This inference is drawn from the fact ascertained by me some time ago, that platina wires acting as electrodes in muriatic acid are polarized by a current so weak as not to be able to electrolyze even iodide of potassium).

e. The most delicate test to ascertain that electrolyzation has taken place, is the polarized state of the electrodes.

I cannot close my letter, Gentlemen, without taking the liberty to point out to you the beautiful, and, as it seems to me, most conclusive evidence in favour of the correctness of the chemical theory of galvanism, now so much contested, which is afforded by the fact stated in § 10. If the mere contact of the two different fluids mentioned there were the real cause of the current obtained, it is obvious that the same current ought to be produced whether the fluid be connected with the galvanometer by means of gold, or if they be connected with the instrument by that of platina wires; but the result being determined by the nature of the connecting wires, and platina being known to favour the union of hydrogen and oxygen, whilst gold and silver do not possess in any sensible degree that property, we are entitled to assert that the current in question is caused by the combination of hydrogen with (the) oxygen (contained dissolved in water) and not by contact.

I am, Gentlemen, yours, &c.

Bâle, Dec. 1838. C. F. Schœnbein.

XI. *On the Formulæ representing Chabasie, in a Letter to Professor* Johnston. *By* R. Phillips, *F.R.SS., L. & E.*

My dear Johnston,

To your paper, contained in the last Number of this Journal, "On some apparent exceptions to the law, that like crystalline forms indicate like chemical formulæ," you have appended this note: "If Mr. Phillips will look at the formula for chabasie given in the table, his difficulty about the mutual replacement of potash and soda *will* or *ought to* disappear."

Before however doubts can cease to exist, clear grounds must be stated for their removal; allow me therefore to direct your attention to the very different statements which you have made at various and not very distant periods, respecting the composition of chabasie.

In the London and Edinburgh Philosophical Magazine, vol. ix. p. 168, you observe that "chemists have recognised two varieties of chabasie; one from Aussig in Bohemia, and from Fassa, analysed by Hoffman, and from Faroe analysed by Arfvredson; represented by the formula,

$$\left.\begin{matrix} C \\ N \\ K \end{matrix}\right\} S^2 + 3\,A\,S^2 + 6\,Aq,$$

and another from Nova Scotia, analysed by Hoffman, from Gustafsberg, by Berzelius, and from Kilmacolm, by Mr. Connell, represented by the formula,

$$\left.\begin{matrix} C \\ N \\ K \end{matrix}\right\} S^3 + 3\,A\,S^2 + 6\,Aq."$$

As *chemical* formulæ the above have no meaning; the latter, I presume, was intended to include the *mineralogical* formula of lime chabasie, as given by Berzelius, *Nouveau Système de Mineralogie* (p. 219), $\dot{C}S^3 + 3\,AS^2 + 6\,Aq$. But you have omitted to adopt a necessary precaution in employing mineralogical formulæ; for Berzelius says, "pour éviter toute espèce de confusion, qui pourrait provenir des deux sortes de formules, je marquerai les minéralogiques en caractères italiques."

In the Report of the British Association, vol. vi. p. 176, you have thus represented

$$\text{“ Chabasie } \left.\begin{matrix} \dot{Na}^3 \\ \dot{K}^3 \\ \dot{Ca}^3 \end{matrix}\right\} \dddot{Si}^2 + 3\ddot{Al}\ddddot{Si}^2 + 18\dot{H}.\text{”}$$

In the above formula, owing, as I suppose, to the slipping of one of the marks representing an equivalent of oxygen,

aluminium looks like a binoxide and silicium a quadroxide. I believe that three of the dots were intended for the Al, and three for the Si; if I am wrong you will be good enough to settle this *point**.

Lastly, in the London and Edinb. Phil. Mag. for December, you have given as the formula for

$$\text{“Chabasie } \left.\begin{matrix}\dot{\mathrm{C}}\mathrm{a}^3\\ \dot{\mathrm{N}}\mathrm{a}^3\\ \dot{\mathrm{K}}^3\end{matrix}\right\} \dddot{\mathrm{S}}\mathrm{i}^2 + 3\,\underline{\dddot{\mathrm{A}}\mathrm{l}}\,\dddot{\mathrm{S}}\mathrm{i}^2 + 10\,\dot{\mathrm{H}}.\text{”}$$

Now mark the differences of these formulæ: in the two first we find 6 atoms of water, in the third 18, and the fourth 10 atoms. $\dddot{\mathrm{A}}\mathrm{l}$ *intended* in the third formula means teroxide of aluminium, but I imagine it ought to have been $\underline{\dddot{\mathrm{A}}\mathrm{l}}$, a sesquioxide, as in the fourth formula*.

You will greatly diminish the difficulties which you have thus created by favouring me with the following statements:

The weight of the atom of lime, potash, soda, aluminium, alumina, silicium, silica, and water, the symbol of each, the number of atoms of each contained in chabasie, and the mode in which you consider that the various silicates which chabasie contains are constituted, with their atomic weights and symbols.

When you have complied with the above request, I will endeavour to *see what I ought*; but at present, with data to be collected from four discordant formulæ, I see nothing but confusion. I remain,

My dear Johnston, yours faithfully,

London, Dec. 12, 1838. R. PHILLIPS.

XII. *Note by Professor* J. J. Sylvester *on his former Paper inserted in the London and Edinburgh Philosophical Magazine for December*, 1838.

University College, London, Dec. 13, 1838.

I HAVE to apologize for calling “original” (in the last Number of the Magazine) the theorem of numbers which I termed “a pendent to Horner's theorem.” This Mr. Ivory has done me the honour to inform me may be found in Gauss's *Disquisitiones Arithmeticæ*, page 76. As Horner's extension

* As evincing the extent to which an accident (against which no care of the printer can always sufficiently guard) may occur in this mode of symbolizing oxygen, I may mention that in the *proof* of this communication, the three dots intended for this Al, had retrograded to the previous word.

of Fermat's theorem suggested this extension of Sir John Wilson's to me, so I concluded that had this extension of Wilson's been known to the world it would naturally have suggested his to Horner. No acknowledgement of this kind having been made, I took it for granted that the theorem I gave was new. Undoubtedly had Mr. Horner been aware of Gauss's theorem he would have made mention of it. My acquaintance with Gauss's principle has not been derived from the study of his works, but from a casual statement of it in an English work, "Dynamics," by Mr. Earnshaw, of St. John's College, Cambridge.

XIII. *Proceedings of Learned Societies.*

GEOLOGICAL SOCIETY.

Nov. 7, 1838.—A paper was first read, On some Fossil Remains of Palæotherium, Anoplotherium, and Chæropotamus, from the freshwater beds of the Isle of Wight, by Richard Owen, Esq., F.G.S., Hunterian Professor in the Royal College of Surgeons.

Some years previous to 1825, Mr. Thomas Allan, of Edinburgh, found in the freshwater beds at Binstead in the Isle of Wight, a tooth which was subsequently determined by Mr. Pentland, to be a molar of the *Anoplotherium commune* *; and in 1830, Mr. Pratt found in the same quarries teeth of one species of Anoplotherium and of two species of Palæotherium †; and thus the freshwater strata of the Hampshire basin were proved to contain remains of some of the Pachydermata which had been discovered in the gypsum quarries of Montmartre.

The specimens described by Mr. Owen in this paper were collected by the Rev. W. Darwin Fox, at Binstead and Seafield; and being numerous and well preserved they have enabled the author to establish a still greater agreement in the remains of the two localities. Of the genus Palæotherium, the collection contained teeth and bones of *P. medium*, *P. crassum*, *P. curtum?* and *P. minus*; and of the genus Anoplotherium, teeth of *A. commune* and *A. secundarium*.

The most important specimen, however, in the collection is a right ramus of the lower jaw of the Chæropotamus, wanting only one false molar, a small portion of the symphysis, and the top of the coronoid process.

This genus was established by Cuvier from an imperfect fragment of the base of the skull, with six molar teeth on each side, and a small portion of a ramus of the lower jaw with the canine? and two spurious molars. He nevertheless proved from the form of the teeth, the glenoid cavity, and the zygomatic arches, that the animal belonged to the Pachydermata and was most nearly allied to the Pec-

* See a paper by Dr. Buckland, Annals Phil. New Series, vol. x. p. 360.
† Geological Transactions, Sec. Ser., vol. iii. p. 451.

cari. In some points, however, in which these remains deviate from the Peccari, they were shown by Mr. Owen to indicate an approach to the carnivorous type, and this affinity he showed is further exhibited in the specimen found by Mr. Fox, in the prolongation backward of the angle of the jaw, a character which in the class Mammalia has hitherto been found almost exclusively in the carnivorous order, and certainly in no Pachydermatous or other ungulate species of Mammal. In the jaw from the Isle of Wight the angle is more compressed and deeper than in the bear, dog, or cat tribe; and it is not bent inwards in the way which peculiarly distinguishes the marsupial jaws, and which so neatly characterizes the Stonesfield mammiferous remains. The condyloid process in the Chæropotamus is raised higher above the angle of the jaw than in the true Carnivora; and it is less convex than in the hog or peccari; and the coronoid process is more developed than in the peccari. In the wavy outline of the inferior border of the lower jaw, and in the teeth, which are well developed in the jaw described by Mr. Owen, a close resemblance is displayed in the Chæropotamus to the peccari. The jaw contains three true tuberculated molars and three conical false molars with double fangs, which molars are relatively larger than in existing Suidæ, and an anterior tooth, which Cuvier in the Paris basin specimens considered to be a canine, but which is situated closer to the symphysis of the jaw than in any of the Suidæ.

Mr. Owen then observed, that the occasional canine propensities of the common hog are well known; and that they correspond with the organization of the genus which offers the nearest resemblance among the existing Pachydermata to the carnivorous type of structure. In the extinct Chæropotamus we have evidently another of those beautiful examples in Palæontology of links tending to complete a chain of affinities which the revolutions of the earth's surface has interrupted, and for a time concealed from our view. It is interesting also to perceive that the living sub-genus of the hog-tribe which most resembles the Chæropotamus should be confined to the South American continent, where the Tapir, the nearest living analogue of the Anoplotherian and Palæotherian associates of the Chæropotamus, now exists.

The author then offered some remarks on a jaw discovered by Mr. Pratt in the Binstead quarries in 1830, and considered by him to be allied to the genus Moschus*. On comparing the jaw with the corresponding part of the *Moschus moschiferus*, which it resembles in size, Mr. Owen has found that in the fossil the grinders are relatively broader, that the last molar has the third or posterior tubercle divided by a longitudinal fissure, that the grinding surface is less oblique, and that the coronoid process differs from that of the Moschus and other ruminants, but strongly bespeaks an affinity with the Pachydermata.

Among the genera of the Paris basin established by Cuvier, the Dichobune exhibits characters which connect the Pachydermata with

* Geological Transactions, Sec. Ser. vol. iii. p. 451.

the Ruminantia, and thus exhibits another of those extraordinary unions of characters which in existing mammalia belong to distinct orders. In the Dichobune the posterior molars begin to exhibit a double series of cusps, of which the external present the crescentic form, so that the teeth of the *Dichobune murina* might be mistaken for those of true Ruminantia. In the lower jaw of the Dichobune the antepenultimate and the penultimate grinders have two pairs of cusps, and last grinder three pairs, of which the posterior are small and almost blended together, so that when worn down they appear single.

In this respect, as well as in the form of the ascending ramus of the lower jaw, Cuvier states, in the Ossemens Fossiles, that the Dichobune "prodigiously resembles" the young Musk Deer.

Now with respect to Mr. Pratt's specimen, Professor Owen observed, there is undoubtedly a close resemblance to the Musk Deer, but the differences are sufficiently great to forbid its being placed among the Ruminantia, while there is a still nearer resemblance between it and the genus Dichobune. The Isle of Wight specimen being somewhat larger than the *D. leporinum*, and the ascending ramus differing in form and approaching that of the true Anoplotheria, Mr. Owen considers that it indicates a new species, which until the form of the anterior molars and incisors are known, may be referred to the genus Dichobune, under the name of *Dichobune cervinum*.

A memoir on the drift from the chalk and strata below the chalk in the counties of Norfolk, Suffolk, Essex, Cambridge, Huntingdon, Bedford, Hertford, and Middlesex, by James Mitchell, Esq., LL.D., F.G.S., was then read.

The drift which is so extensively distributed over the above counties, consists chiefly of stiff blue and yellow clay, varying from 4 feet to above 70 in thickness; and it contains masses and small fragments of chalk, chalk flints, primary, secondary and other rocks, and fossils from nearly every secondary formation in England. In some localities the clay forms the mass of the drift, but in others it contains or rests on beds of sand and gravel; and it is often overlaid by a deposit, occasionally exceeding 50 feet thick, of sand, gravel, and chalk flints.

The principal locality in Norfolk, mentioned by the author, is Cromer, the cliffs near which vary in height from 100 to 150 feet; the lower half consisting of blue clay charged with masses and fragments of chalk, unaltered chalk flints, and secondary and primary rocks; and the upper half of sand and gravel, capped by 2 feet of ferruginous sand, in some places black. The same general description, it is stated, will apply to the cliffs for 12 miles east and west of Cromer; but they occasionally present most extraordinary contortions of the beds. The other localities in Norfolk, alluded to by the author, are in the parishes of Pulham St. Mary Magdalen, Pulham St. Mary the Virgin; and a pit one mile from Harleston towards Diss, where 4 feet of blue clay, abounding with chalk pebbles, are overlaid by 2 feet and underlaid by 10 feet of gravel and flints: the author also states, that the clay with chalk pebbles ex-

tends between Harleston and Diss, the latter town and North Lopham, and thence to Norwich, Dereham, and Swaffham. In Suffolk it was examined by him at Lowestoff, particularly in the cliff on the north side of the town, where he obtained the following section:

Covered slope	15 feet.
Black sand	1 —
Red and yellow sand	15 —
Blue clay, with fragments of chalk, chalk flints, oolite, and lias	12 —
Red and yellow sand	2 —
Covered slope	20 —
	65

In the sea cliff a quarter of a mile north of Southwold, in Suffolk, the clay contains a bed of sand two feet thick; in the same county he likewise noticed it near Woodbridge, between Wrentham and Wangford, and near the road from Wangford to Southwold. The localities in Essex mentioned by the author are Maldon, Kelvedon, Braintree, Castle Heddingham near Halstead, Navestock, and Upminster; in Cambridgeshire, Ely and between Caxton and Arrington; in Huntingdonshire, the districts between Huntingdon and Peterborough and Huntingdon and Caxton; in Bedfordshire, Castle Hill, 6 miles east of Bedford; in Buckinghamshire, the line of the London and Birmingham railway, near Fenny Stratford and Leighton Buzzard, where it rests on the lower greensand, and is overlaid by gravel containing rounded fragments of ferruginous sandstone; in Middlesex the only localities mentioned are Finchley and Muswell Hill; in Hertfordshire the clay was not noticed by the author, though the gravel abounds with fragments of secondary and other formations.

A description is then given of the transported rocks either inclosed in the clay or accumulated in beds of gravel. They consist of hard and soft chalk, flints, oolite, cornbrash, lias, sandstones, mountain limestone, mica slate, trap, granite, syenite, porphyry, &c. The principal localities mentioned are the Stags Inn near Diss, the Holywell and Witlingham near Norwich, Ballingdon Hill near Sudbury; between Peterborough and Huntingdon and thence to Caxton, also between that place and Arrington; in Hertfordshire these accumulations are said to abound around Buntingford, Hare Street, Puckeridge, Much Haddam, and Newnham near Baldock: a few specimens occur around Hertford and at Ware Mill, and Wade's Mill, 1½ miles from Ware. The pits at Muswell are particularly noticed, and the collections from them formed by Mr. Wetherell and Mr. Frederick Pusey; but the specimens of rocks are said to be not nearly so numerous, nor the size of the masses so great as in Hertfordshire, Huntingdonshire, Suffolk, and Norfolk.

Besides the smaller fragments two large boulders are described. One consisting of granite and computed to weigh a ton and a half, lies by the road side on the north of the village of Hare Street; and it is so thoroughly rounded that the author had great difficulty in

detaching a fragment. The other boulder occurs at Baldock, and consists of hard chalk containing common black flints. It is about 3 feet 9 inches high above ground, $2\frac{1}{2}$ feet long, and nearly 2 feet broad.

The current by which the drift was accumulated, the author conceives came from a point to the east of north, and he is of opinion that the materials have been derived in part from Scandinavia and in part from the destruction of strata, which once occupied the site of the German Ocean. After the deposition of the clay, Dr. Mitchell believes, that there was a violent action which accumulated the beds of gravel in some places to the depth of above 100 feet (Beaumont Green, 110 feet; the Isle of Dogs, 124 feet); and that this action will account for the clay not being found in more places, and being occasionally associated with beds of gravel.

The paper concludes with a slight allusion to a similar north-east drift, north of the counties enumerated in the title; and it is stated that grey quartz boulders continue to be thrown in at Spurn Head, Yorkshire, similar to those which are found in some of the vales of the counties of Lincoln, Nottingham, and Leicester. Fragments of mountain limestone, lias, oolite, grey quartz, white quartz, and hard chalk are said to occur about Mount Sorrell.

ROYAL SOCIETY.

[Continued from vol. xiii. p. 467.]

November 15, 1838.—A paper was read, entitled, "Discovery of the Source of the Oxus." By Lieut. Wood, of the Indian Navy. Communicated by James Burnes, K.H., D.C.L., F.R.S., in a letter to the Secretary of the Royal Society.

The following notice of the discovery of the source of the Oxus by Lieut. Wood, one of the officers serving under Captain Alexander Burnes, F.R.S., in his political and scientific mission to Cabul, is contained in a letter from Captain Burnes:

"This celebrated river" (the Oxus) "rises in the elevated region of Pameer in Sinkoal. It issues from a sheet of water, encircled on all sides, except the west, by hills, through which the infant river runs; commencing its course at the great elevation of about 15,600 feet above the level of the sea, or within a few feet of the height of Mont Blanc. To this sheet of water Lieut. Wood proposes to assign the name of *Lake Victoria*, in honour of Her Majesty."

November 22, 1838.—A paper was read, entitled, "On the State of the Interior of the Earth." By W. Hopkins, Esq., M.A., F.R.S., F.R.A.S., &c.

The object of the present memoir is to inquire into the modes in which the refrigeration of the earth may have taken place, on the hypothesis that its entire mass was originally in a fluid state; an hypothesis which was at first founded on astronomical considerations, and is now corroborated by the discoveries of modern geology, exhibiting the apparent injection from below of large masses of unstratified rocks, through the fissures of sedimentary strata. As-

suming that this state of fluidity was the effect of heat, we are led to consider the steps of transition by which the earth has passed into its present state of solidity, and apparently permanent temperature. After adverting to the analytical investigations of Fourier and Poisson on this subject, the author proceeds to inquire into the results of the laws of refrigeration of heated bodies, which may be conceived to operate in the present case; namely, refrigeration by *circulation*, which obtains when the fluidity is perfect, and that by *conduction*, when the particles of the mass, by the diminution of fluidity, no longer retain that mobility among one another which is requisite for their circulation. Thus while, in either case, the superficial parts of the earth would rapidly cool and solidify by the radiation of their heat into sidereal space, forming a crust of small thickness compared with the whole radius of the globe, the internal mass may be in one or other of the three following conditions:—*First*, it may consist of matter still in a state of fusion, of which both the temperature and the fluidity are greatest at the centre, but which has been brought, by the long-continued process of circulation, into a state no longer admitting of this process, and capable, therefore, of cooling only by conduction. *Secondly*, the earth may consist of an external shell, of a central nucleus, rendered solid by the enormous pressure to which it is subjected, and of an intermediate stratum of matter in a state of fusion. The thickness of the shell, as well as the radius of the solid nucleus, may possibly be small compared with the radius of the earth. The fluidity of the intervening mass must necessarily be here, also, considerably more imperfect than that which would just admit of cooling by circulation. *Thirdly*, the earth may be solid from the surface to the centre.

The author then shows that the direct investigation of the manner in which the earth has been cooled, assuming its original fluidity from heat, cannot determine the actual condition of its central parts, not from any imperfection in the analytical process, but from the want of the experimental determination of certain values, which it is extremely difficult, if not impossible, accurately to obtain. It has occurred to the author that a more indirect test of the truth of the hypothesis of the central fluidity of the earth might be found in the delicate but well-defined phenomena of precession and nutation. The investigation of the problems thus suggested is reserved by the author for the subject of a future memoir.

Anniversary, November 30, 1838.—The lists of Fellows deceased, and of Fellows admitted since the last Anniversary having been read, it was stated that the report of the death of R. Z. Mudge, Capt. R.E., noticed at the last Anniversary, has been since found to be erroneous.

The following Address of His Royal Highness the President was read from the Chair:

Gentlemen,

I cannot quit the Chair of the Royal Society, which I have now occupied during a period of eight years, without availing myself of the opportunity which the customary proceedings of the Anniver-

sary afford me, of expressing to you the grateful sense I entertain of the great honour conferred upon me, by being chosen to fill so distinguished an office, as likewise of the uniform kindness and support which I have always received from the Members of the Council and the Fellows of the Society generally, in the discharge of its various and important duties.

A review of my conduct during the period of my Presidency, recalls to my mind many occasions in which I am sensible that I have been more or less wanting in the very responsible trust confided to me, of watching over the interests of a Society most justly illustrious by the succession of great men who have been connected with it and by the great advances which nearly every department of science has received from those portions of their labours which are recorded in its Transactions; for *some* of these deficiencies I am unfortunately enabled to refer to the severe and long continued visitations of disease and infirmity under which I have laboured, as a very sufficient apology; and I feel less oppressed than I otherwise should have been, by my consciousness of many others, by my knowledge of the activity and zeal of the very able and efficient officers upon whom the temporary discharge of my duties devolved, and from the assurance which I felt, that the interests of the Society, when entrusted to their care, would suffer no detriment by my absence.

Though justly proud of the distinction of presiding over the Royal Society, and most anxious to promote, to the utmost of my power, the great objects for which it was founded, I no sooner ascertained that circumstances would probably, for a time, interfere with my residence in London, during a considerable part of its Annual Session, and prevent my receiving its Members in a manner compatible with my rank and position in this country, than I determined to retire from an office whose duties I could no longer flatter myself as likely to be able to discharge in a manner answerable to their expectations, or in accordance with my own feelings. Having come to this conclusion after the most anxious and painful consideration, I deemed it due to the Members of the Council, in the first instance, and next to the Fellows, to make it speedily and generally known, with the view of enabling them to look out for a proper person to fill a situation of such dignity in the scientific world, and whose occupation could not fail to be an object of honourable ambition to men of the most eminent social rank, as well as of the most distinguished scientific attainments.

I will not attempt to disguise from you, Gentlemen, the feelings of deep and poignant regret I experienced upon taking a step that would thus necessarily abridge the opportunities, which I had as much enjoyed as I had highly prized, of being brought officially into frequent and familiar contact with the most distinguished philosophers of my own or other countries, and of employing whatever influence my station in society enabled me to exert in advocating the just claims and interests of men of science, in promoting the objects of their labours, in fostering and encouraging their mutual co-operation and intercourse, and in endeavouring to

soothe the violence of personal or national jealousies, whenever they unfortunately existed, by bringing them together in social or other meetings where the discussion of topics of irritation could be either suppressed or controlled, and where imaginary prejudices would disappear under the softening operation of reciprocal knowledge and experience. But though deprived for a season, by my retirement, of some of the highest privileges I have hitherto exercised and enjoyed, yet I do not abandon the hope of being still able to maintain and cultivate the very valuable and delightful friendships which I have thus fortunately for myself been enabled to form during the period of my connection with you, by seizing every occasion when presented to me, of appearing at the meetings of the Royal Society, and by co-operating with its members, to the utmost extent of my limited means, in furthering those objects that may be considered to be most important for the advancement of the interests of science.

I am afraid however, Gentlemen, that I have already trespassed unreasonably upon your time and attention in endeavouring to explain to you the motives of my conduct, and to express, though most inadequately, my grateful sense of the kindness which I have invariably experienced from you. I shall therefore now proceed to the more immediate subject of this Address, which is to notice some of the most important Proceedings of the Society which have taken place during the last year.

The Address voted to Her Majesty by the President and Council of the Royal Society, on the Queen's accession to the throne, embodying likewise a petition to Her Majesty to become the Patron of the Society, and to continue to it the Grant of the Medals which had been instituted by King George the Fourth and regranted by William the Fourth, as well as the gracious reply of the Sovereign, transmitted through the Secretary of State for the Home Department, have been already communicated to you at one of the weekly meetings of the Society*. On the 20th of June last, the President and Council were summoned to attend at the Palace of St. James's to witness Her Majesty's signature in our Charter-Book as Patron of the Society. I availed myself of the occasion thus presented to me to address the Queen in your name, and to assure Her Majesty that we felt bound by the obligations of our Charter, as well as by the recollection of our foundation, to look up to the Sovereign of these realms as our Patron and protector: that we most gratefully acknowledged the assurances which Her Majesty had conveyed to us through Her minister the Secretary of State for the Home Department, of the continuance of the same support and favour as had been always accorded to us by the Sovereigns of this Kingdom, and likewise the signification of Her Majesty's intention of renewing the grant of the two Medals which had been instituted by one and confirmed by another of Her Majesty's royal uncles and predecessors, accompanied by Her gracious permission to propose such modification and amendments in the statutes which

* June 21, 1838.

had been provided for their distribution, as would tend most effectually to promote the advancement of science, and would most certainly accomplish the liberal and patriotic views and intentions of their Royal Founders. I further ventured to advert to the close connection which exists between the cultivation of Science and the Arts, and the progress and developement of the great elements of the prosperity and happiness of nations, and to express my earnest hope and prayer that the triumphs of the arts of peace and commerce, which had so signally marked the beginning of Her Majesty's reign, might be continued without intermission to its distant conclusion.

The Queen having received the Address in the most gracious manner, was pleased to sign her august and royal name in our Charter-Book as Patron of the Royal Society: after which the officers and different members of the Council were presented by me to Her Majesty, and had the honour of kissing Her Majesty's hand.

The alterations in the laws for the distribution of the Royal Medals, which Her Majesty was graciously pleased to authorize and permit, have been made by a Committee of the Council appointed for that purpose, and have since received the especial sanction and approbation of Her Majesty. They are directed to be given hereafter to such papers, and to such papers only, as have been presented to the Society, or inserted in its Transactions, within three years of the date of the award; and they are to be awarded to departments of science whose order of succession is defined by a cycle of three years, comprising in the first *Astronomy* and *Physiology*, in the second *Physics* and *Geology*, and in the third *Mathematics* and *Chemistry*. And it is further added and commanded, that no departure from this order of succession shall be allowed, unless it shall appear that no memoir of sufficient merit to be entitled to such an honour shall have been presented to the Society within the period afore-named; in which case, and in which case only, it shall be competent for the Council, with the approbation of Her Majesty, to award the Medal to one of those branches of science which are comprehended in the cycle of the preceding year.

I trust, Gentlemen, that these laws for the distribution of the Royal Medals, if strictly adhered to, and judiciously administered, will be found to stimulate the exertions of men of science, by securing to their labours, when inserted in our Transactions, that certain and periodical revision which they are naturally so anxious to obtain; and by signalizing any remarkable investigation, or notable discovery, by the marked and prompt approbation of those persons in this country who are most likely to be able to judge of its value.

It was partly for the furtherance of the same great object, which was proposed in framing the statutes for the award of the Royal Medals, so as to secure to each branch of science in succession its due amount of notice and encouragement, that the Council have determined to establish permanent Committees of Science. They

are composed of a selection of those Fellows of the Society who are known to have devoted their attention, in a more especial manner, to those departments of science to which they are severally assigned, and to whom all questions connected with such branches are proposed to be referred, including the selection of the memoirs to which the Royal Medals shall be given. The Council have thought proper, likewise, in the formation of these committees, to enlarge the number of the sciences, which form the Medallic cycle above referred to, from six to eight, by separating the science of Meteorology from that of Physics, and the science of Botany and the laws of Vegetable Organization and Life, from that of Zoology and Animal Physiology. I sincerely rejoice, Gentlemen, in the adoption of this arrangement, as I think it admirably calculated to give a more marked and specific distinction to those sciences which the Fellows of the Royal Society are bound more especially, by the obligations of the Charter, to cultivate, and as tending, likewise, to bring those persons who are engaged in common pursuits into more frequent intercourse with each other; and thus to afford them increased opportunities of appreciating their mutual labours, of devising new and important trains of investigation, as well as of securing public aid and general co-operation in the accomplishment of objects which are too costly or too vast for individuals to undertake or to attempt.

The future developement of many of the sciences is becoming daily more and more dependent upon co-operative labour. We are rapidly approaching great and comprehensive generalizations, which can only be completely established or disproved by very widely distributed and, in many cases, by absolutely simultaneous observations. Major Sabine has lately collected with great labour, and reduced and analysed with great ability, a vast mass of observations relating to the distribution of the earth's magnetism; and the result has pointed out not merely the proper fields of our future researches, but likewise their great extent and the enormous amount of labour still required for their cultivation. A society on the continent, headed by the justly celebrated Gauss, to whom the Copley Medal has been this year adjudged for his magnetical researches, my cotemporary and fellow student at Göttingen, has instituted a system of simultaneous observations on the periodical and irregular movements of the magnetic needle at various stations in different parts of Europe, which suggest conclusions of the most surprising and interesting nature; these can only be fully worked out and confirmed by the adoption of a similar system of observations in places extremely remote from each other on the surface of the globe. The researches on the tides, which have been so laboriously and so successfully prosecuted by Professor Whewell and Mr. Lubbock, have led, and can lead to few general and certain conclusions without the aid of labours of this nature; and a memorable exemplification of their value, even when given in their rudest and least perfect form*, is presented in the

* From the logs of ships.

discovery of the "Law of Storms," which Col. Reid has recently published, and which promises results so important to the interests of navigation and the cause of humanity. In the science of Meteorology, which still remains destitute even of approximations to general laws, it is to a well-organized system of simultaneous observations that we must look for the acquisition of such a knowledge of the range and character of atmospheric influences and changes, as may become the basis of a well-compacted and consistent theory, and rescue this science from the reproach, under which it has too long and too justly laboured, of presenting little more than a confused mass of almost entirely insulated results. Undertakings, however, of this extensive and laborious nature are far beyond the reach of individual enterprize, and can only be accomplished by national aid and co-operation.

We have lately witnessed an example where the Storthing, or National Assembly of Norway, a body composed partly of peasants, and representing one of the poorest countries in Europe, undertook the charge of a magnetical expedition to Siberia, on the recommendation and under the direction of their distinguished countryman, M. Hansteen, at the same time that they refused a grant of money to aid in building a palace for their sovereign; and I feel confident that the united wishes of men of science in this and other countries, whose influence on public opinion is becoming daily more and more manifest, particularly when expressed in favour of purely scientific objects which cannot be effected without the assistance and the resources of the nation, will not be without their effect on the Government of our own country, which has always taken the lead in the promotion of geographical as well as scientific investigations and discoveries, and which possesses, beyond any other nation, advantages for their prosecution and accomplishment, not merely from its superior wealth, but from the range and distribution of its commerce and its colonies in every region of the globe.

There is one other event to which I wish to advert previously to concluding this portion of my address to you, and which I conceive I may do with the strictest propriety, as it is closely connected with the general interests of the Royal Society. I allude to the return of Sir John Herschel to this country, after an absence of several years, devoted, from a sense of filial duty, to the completion of that great task which he felt to have been transmitted to him as an inheritance from his venerable and illustrious father. I have so often had occasion to allude, from this Chair, to the merits of that distinguished person, and to express the respect which I felt for his great attainments, the pride with which I cherished his friendship, the deep interest which I took in his labours, and my admiration of the truly modest and philosophical spirit in which they were conducted, that I should be guilty of a very superfluous repetition of what I have before addressed to you, if I ventured to enlarge upon them now; but I should ill discharge my duty, whilst still entitled to address you as the official head of the scientific establishment of this country, if I omitted to avail myself of this or any other opportunity of

expressing the gratification which I experienced in June last, when called upon to preside at that great convention of the most eminent men who adorn our country, who combined together with such singular unanimity and enthusiasm to pay their homage to science and knowledge, and those great interests with which their cultivation and progress are connected, by paying so signal a tribute of respect and honour to the most accomplished and the most devoted of our living philosophers. I feel assured, Gentlemen, that the proceedings of that memorable day will produce marked and durable effects upon the scientific prospects of our country, by proving that pre-eminent merit will meet with sympathy at least, if not with reward, and as offering sure and unequivocal indications both of the power and direction of public opinion amongst the most cultivated and enlightened classes of society; and it was chiefly as an expression of the deference paid by the government of this country to the opinions and wishes of the scientific world, that I rejoiced in being authorized and requested by the prime minister of the crown to offer to Sir John Herschel the rank of baronet, on the occasion of the coronation of Her Majesty, though well convinced that such an accession of social rank was not required to give dignity to one whose name is written in the imperishable records of the great system of the universe.

It would ill become me, while gratefully acknowledging my sense of your past kindnesses towards myself, to venture to refer to the name of my presumed successor in the Chair of this Society in any terms which might be interpreted as an undue anticipation of the result of this day's proceedings, or as appearing to interfere with the free use of the franchise which every Fellow possesses, and is expected and required to exercise; but I cannot be ignorant of the various accomplishments, the courteous and unassuming manners, the warmth of heart and active benevolence which distinguish the nobleman who has been nominated by the Council: and I rejoice most sincerely that the Society possesses amongst its members, as a candidate for your suffrages, one so well qualified to preside at your meetings, and to watch over your interests.

Amongst the deceased members, I find twenty-seven on the Home, and four on the Foreign list, including some very considerable names. I shall now proceed to notice such of their number as have been most distinguished for their scientific labours, for their public services, or for their encouragement and patronage of science and the arts.

Thomas Andrew Knight, of Downton Castle, Herefordshire, the President of the Horticultural Society of London, to the establishment and success of which he so greatly contributed, was born in the year 1758. He was educated at Ludlow school, and afterwards became a member of Balliol College, Oxford. From his earliest years he appears to have shown a predominant taste for experimental researches in gardening and vegetable physiology, which the immediate and uncontrolled possession of an ample fortune gave him every opportunity of indulging; proposing to himself in fact, as one

of the great objects of his life, to effect improvements in the productions of the vegetable kingdom, by new modes of culture, by the impregnation of different varieties of the same species, and various other expedients, commensurate with those which had already been effected by agriculturists and others in the animal kingdom, by a careful selection of parents, by judicious crossing, and by the avoidance of too close an alliance of breeds. In the year 1795 he contributed to our Transactions his first, and perhaps his most important paper, on the transmission of the diseases of decay and old age of the parent-tree to all its descendants propagated by grafting or layers, being the result of experiments which had already been long continued and very extensively varied, and which developed views of the greatest importance and novelty in the economy of practical gardening, and likewise of very great interest in vegetable physiology. This paper was succeeded by more than twenty others, chiefly written between the years 1799 and 1812, containing the details of his most ingenious and original experimental researches on the ascent and descent of the sap in trees; on the origin and offices of the alburnum and bark; on the phenomena of germination; on the functions of leaves; on the influence of light, and upon many other subjects, constituting a series of facts and of deductions from them, which have exercised the most marked influence upon the progress of our knowledge of this most important department of the laws of vegetable organization and life.

Mr. Knight succeeded Sir Joseph Banks in the presidency of the Horticultural Society, and contributed no fewer than 114 papers to the different volumes of its Transactions: these contributions embrace almost every variety of subject connected with Horticulture; such as the production of new and improved varieties of fruits and vegetables; the adoption of new modes of grafting, planting, and training fruit-trees; the construction of forcing-frames and hot-houses; the economy of bees, and many other questions of practical gardening, presenting the most important results of his very numerous and well-devised experiments.

Mr. Knight was a person of great activity of body and mind, and of singular perseverance and energy in the pursuit of his favourite science: he was a very lucid and agreeable writer, and it would be difficult to name any other cotemporary author in this or other countries who has made such important additions to our knowledge of horticulture and the economy of vegetation.

Sir Richard Colt Hoare, the owner of the beautiful domain of Stourhead in Wiltshire, was the author of many valuable historical and topographical works, and more especially of the history of his native county, presenting so numerous and such splendid funereal and other monuments of the primitive inhabitants of Great Britain, which he investigated with a perseverance and success unrivalled by any other antiquary. The early possession of an ample fortune and of all the luxuries of his noble residence, seems to have stimulated, rather than checked, the more ardent pursuit of those favourite studies, which occupied his almost exclusive attention for more than

fifty years of his life: and he was at all times, both by his co-operation and patronage, ready to aid other labourers in the same field which he had himself cultivated with so much success and industry.

Sir Richard Hoare was a very voluminous original author, and on a great variety of subjects; he printed a catalogue of his unique collection of books relating to the history and topography of Italy, the whole of which he presented to the British Museum, to which he was, on other occasions, a liberal benefactor. He likewise published editions of many of our ancient chronicles; and it is only to be lamented that one who has contributed under so many forms to our knowledge of antiquity, and who presents so many claims to the grateful commemoration of the friends of literature and the arts, should have been influenced so much, and so frequently, by the very unhappy ambition, of which some well-known and distinguished literary bodies of our own time have set so unworthy an example, of giving an artificial value to their publications, by the extreme smallness of the number of copies which they allow to be printed or circulated; thus defeating the very objects of that great invention, whose triumphs were pretended to be the very groundwork of their association.

Mr. George Hibbert was one of the most distinguished of those princely merchants, whose knowledge of literature, patronage of the arts, and extensive intercourse with the world have contributed so much, in a great commercial country like our own, to elevate the rank and character of the class to which they belong, and to give to the pursuits of wealth an enlarged and liberalizing spirit. Mr. Hibbert possessed, during the most active period of his life, an uncommon influence amongst the great commercial bodies of the metropolis, and more particularly amongst those connected with the West India trade, from his integrity and high character, his great knowledge of business, his excellent sense and judgement, and his clearness and readiness in public speaking. He was an excellent botanist, and the collection of plants which he had formed at his residence at Clapham, was remarkable not merely for its great extent, but likewise for the great number of extremely rare plants which it contained. He was well known also as a very extensive and judicious collector of books, prints, drawings and paintings, and was endeared to a large circle of private friends, amongst the most cultivated classes of society in this country, by his refined yet simple manners, his happy temper, and his many social and domestic virtues.

Sir Abraham Hume, who had attained at the time of his death the venerable age of ninety years, was the father of the Royal Society; he was a man of cultivated taste and very extensive acquirements, and throughout his life a liberal patron and encourager of the fine arts.

Lord Farnborough was the son-in-law of Sir Abraham Hume, whom he greatly resembled in his tastes and accomplishments; for more than thirty years of his life he held various public situations in the successive administrations of this country, but quitted his official employments on his elevation to the peerage in 1826:

from that period he devoted himself almost entirely to the improvement and decoration of his beautiful residence at Bromley Hill; to the proposal and promotion of plans for the architectural improvement of the metropolis; to the selection of pictures for the National Gallery, which he greatly enriched by his bequests; and to the various duties imposed upon him by his official connexion with the British Museum, and many other public institutions.

The Earl of Eldon, though possessing few relations with science or literature, presents too remarkable an example of the openings afforded by the institutions of this country to men of great and commanding talents for the attainment of the highest rank and wealth, to be passed over without notice in this obituary of our deceased Fellows. Lord Eldon was matriculated as a member of University College, Oxford, under the tuition of his brother, afterwards Lord Stowell, in 1766; and an academical prize which he gained in the following year, for an "Essay on the Advantages of Foreign Travel," gave the first evidence of his possession of those great powers of minute analysis and careful research, which made him afterwards so celebrated. His early marriage terminated somewhat prematurely his academical prospects, and forced him to adopt the profession of the law, after narrowly escaping other occupations of a much more humble character. He was compelled to struggle for several years of his life with poverty and discouragement, when a fortunate opportunity enabled him to give proof of his extraordinary attainments, and rapidly conducted him to the command of wealth and professional eminence. After filling with great distinction the offices of Solicitor and Attorney-General, he became Chief-Justice of the Common Pleas and a peer in 1799, and finally Lord Chancellor of England in 1801, a situation which he continued to hold, with a short interruption, for nearly a quarter of a century. Of his political character and conduct it becomes not me to speak; but his profound knowledge of the laws of England, his unrivaled acuteness and sagacity, and his perfect impartiality and love of justice, have received the concurrent acknowledgment and admiration of men of all parties.

The Rev. Thomas Catton, Senior Fellow of St. John's College, Cambridge, was in early life a schoolfellow of Lord Nelson, of whose talents or character, however, he retained no very vivid impressions: he became a Member of the University in 1777, and when he took his degree in 1781 he was fourth Wrangler and first Smith's Prizeman, a discrepancy in the results of two similar examinations, which is said to have led to the adoption of some regulations preventing their recurrence in future. In the year 1800 he became one of the public tutors of his college, in conjunction with its present venerable and distinguished master, and secured, in a very uncommon degree, the respect and love of his pupils, by his skill and knowledge as a teacher, and by his kind and vigilant attention to their interests: he quitted the tuition in 1810, and for the remainder of his life he devoted himself, almost exclusively, to the cultivation of practical and theoretical astronomy, having succeeded to Mr. Ludlam in the management of the observatory which is

placed over one of the interior gateways of the college. He possessed a most accurate knowledge of the theory and use of astronomical instruments, and was a most scrupulous and skilful observer; and he is known to have left behind a very large mass of observations, particularly of occultations, most carefully detailed and recorded. Mr. Catton was a man of very courteous manners and most amiable character, and possessed of a very extensive acquaintance both with literature and science. He died in the month of January last, in the eightieth year of his age, deeply regretted by the members of the college in which he had passed the greatest part of his life.

Mr. Henry Earle, one of the Senior Surgeons of St. Bartholomew's Hospital, was the son of one very eminent surgeon, Sir James Earle, and the grandson of another, Mr. Percival Pott. He was the author of many valuable articles in different medical journals, and likewise of two papers in our Transactions; one detailing the result of a very novel and difficult surgical operation, and the other on the mechanism of the spine, which were published in 1822 and 1823. Mr. Earle was considered to be one of the most skilful and scientific surgeons of his age, and was justly esteemed by his professional and other friends not merely for his great acquirements, but for his kindness of heart and upright and honourable character.

John Lloyd Williams, formerly British resident at Benares, was the author of three short papers in our Transactions in the year 1793; two of them upon the method of making ice at Benares, by means of extremely porous and shallow evaporating pans of unglazed earthenware, placed upon dry straw or sugar-cane; and the last furnishing additional descriptions of the great quadrants and gnomon in the observatory at Benares, which had been described in a paper in our Transactions in 1777 by Sir Robert Barker.

The Foreign Members whom the Society has lost during the last year, are Dr. Nathaniel Bowditch, of Boston, in America; Messieurs Dulong and Frederic Cuvier, of Paris; and Dr. Martin van Marum, of Haarlem.

Dr. Nathaniel Bowditch of Boston, in the State of Massachusetts in America, was born at Salem, in the same State, in 1773: he was removed from school at the age of ten years to assist his father in his trade as a cooper, and was indebted for all his subsequent acquisitions, including the Latin and some modern languages and a profound knowledge of mathematics and astronomy, entirely to his own exertions unaided by any instruction whatever. He became afterwards a clerk to a ship-chandler, where his taste for astronomy first showed itself, and was sufficiently advanced to enable him to master the rules for the calculation of a lunar eclipse; and his subsequent occupation as supercargo in a merchant vessel sailing from Salem to the East Indies, led naturally to the further developement of his early tastes, by the active and assiduous study of those departments of that great and comprehensive science which are most immediately subservient to the purposes of navigation. It was owing to the reputation which he had thus acquired for his great knowledge of nautical astronomy, that he was employed by the booksellers to

revise several successive editions of Hamilton Moore's Practical Navigator, which he afterwards replaced by an original work on the same subject, remarkable for the clearness and conciseness of its rules, for its numerous and comprehensive tables, the greatest part of which he had himself recalculated and reframed, and for its perfectly practical character as a manual of navigation: this work, which has been republished in this country, has been for many years almost exclusively used in the United States of America.

Dr. Bowditch having been early elected a Fellow of the American Academy of Arts and Sciences at Boston, commenced the publication of a series of communications in the Memoirs of that Society, which speedily established his reputation as one of the first astronomers and mathematicians of America, and attracted likewise the favourable notice of men of science in Europe.

During the last twenty years of his life, Dr. Bowditch was employed as the acting president of an Insurance Company at Salem, and latterly also as actuary of the Massachusetts Hospital Life Insurance Company at Boston: the income which he derived from these employments, and from the savings of former years, enabled him to abandon all other and more absorbing engagements, and to devote his leisure hours entirely to scientific pursuits. In 1815 he began his great work, the translation of the *Mécanique Céleste* of Laplace, the fourth and last volume of which was not quite completed at the time of his death. The American Academy over which he presided for many years, at a very early period of the progress of this very extensive and costly undertaking, very liberally offered to defray the expense of printing it; but he preferred to publish it from his own very limited means, and to dedicate it as a splendid and durable monument of his own labours and of the state of science in his country. He died in March last, in the sixty-fifth year of his age, after a life of singular usefulness and most laborious exertion, in the full enjoyment of every honour which his grateful countrymen in every part of America could pay to so distinguished a fellow-citizen.

Dr. Bowditch's translation of the great work of Laplace is a production of much labour and of no ordinary merit: every person who is acquainted with the original must be aware of the great number of steps in the demonstrations which are left unsupplied, in many cases comprehending the entire processes which connect the enunciation of the propositions with the conclusions, and the constant reference which is made, both tacit and expressed, to results and principles, both analytical and mechanical, which are co-extensive with the entire range of known mathematical science: but in Dr. Bowditch's very elaborate commentary every deficient step is supplied, every suppressed demonstration is introduced, every reference explained and illustrated, and a work which the labours of an ordinary life could hardly master, is rendered accessible to every reader who is acquainted with the principles of the differential and integral calculus, and in possession of even an elementary knowledge of statical and dynamical principles.

When we consider the circumstances of Dr. Bowditch's early life,

the obstacles which opposed his progress, the steady perseverance with which he overcame them, and the courage with which he ventured to expose the mysterious treasures of that sealed book, which had hitherto only been approached by those whose way had been cleared for them by a systematic and regular mathematical education, we shall be fully justified in pronouncing him to have been a most remarkable example of the pursuit of knowledge under difficulties, and well worthy of the enthusiastic respect and admiration of his countrymen, whose triumphs in the fields of practical science have fully equalled, if not surpassed, the noblest works of the ancient world.

Pierre Louis Dulong was born at Paris in 1785: he became an orphan at the age of four years; and though hardly possessing the most ordinary advantages of domestic instruction or public education, his premature talents and industry gained him admission at the age of 16 to the Polytechnic School, which has been so fertile in the production of great men, of which he became afterwards successively examiner, professor, and director. He first followed the profession of medicine, which he abandoned on being appointed Professor of Chemistry to the Faculty of Sciences. He became a member of the Institute in 1823, in the Section of the physical sciences. On the death of the elder Cuvier he was appointed Secretaire Perpetuel to the Institute, a situation from which he was afterwards compelled to retire by the pressure of those infirmities which terminated in his death in the fifty-fourth year of his age.

M. Dulong was almost equally distinguished for his profound knowledge of chemistry and of physical philosophy. His "Researches on the mutual decomposition of the soluble and insoluble Salts," form a most important contribution to our knowledge of chemical statics. He was the discoverer of the *hypophosphorous acid*, and also of the *chlorure of azote*, the most dangerous of chemical compounds, and his experiments upon it were prosecuted with a courage nearly allied to rashness, which twice exposed his life to serious danger; and his memoirs on the "Combinations of phosphorus with oxygen," on the "*hyponitric acid*," on the *oxalic acid*, and other subjects, are sufficient to establish his character as a most ingenious and accurate experimenter, and as a chemical philosopher of the highest order.

But it is to his researches on the "Law of the conduction of heat," "On the specific heat of the gases," and "On the elastic force of steam at high temperatures," that his permanent fame as a philosopher will rest most securely: the first of these inquiries, which were undertaken in conjunction with the late M. Petit, was published in 1817; and presents an admirable example of the combination of well-directed and most laborious and patient experiment with most sagacious and careful induction: these researches terminated, as is well known, in the very important correction of the celebrated law of conduction, which Newton had announced in the Principia, and which Laplace, Poisson, and Fourier had taken as the basis of their beautiful mathematical theories of the propagation of heat. His ex-

periments on the elastic force of steam at high temperatures, and which were full of danger and difficulty, were undertaken at the request of the Institute, and furnish results of the highest practical value; and though the conclusions deduced from his "Researches on the specific heat of gases" have not generally been admitted by chemical and physical philosophers, the memoir which contains them is replete with ingenious and novel speculations, which show a profound knowledge and familiar command of almost every department of physical science.

M. Frederic Cuvier, the younger brother of the illustrious Baron Cuvier, Professor of Animal Physiology to the Museum of Natural History at Paris, and Inspector-general of the University, was born at Montbelliard, in Alsace, in 1773: he had from an early period attached himself to those studies which his brother had cultivated with so much success, and his appointment as keeper of the menagerie at the Jardin des Plantes, furnished him with the most favourable opportunities of studying the habits of animals, and of prosecuting his researches on their physiology and structure. The Annales d'Histoire Naturelle, and the Mémoires du Muséum, contain a series of his memoirs on zoological subjects of great value and interest, and his work "*Sur les Dens des Mammifères considerées comme Caractères Zoologiques,*" is full of novel and original views and observations, and has always been considered as one of the most valuable contributions to the science of Zoology which has been made in later times: the great work "*Sur l'Histoire des Mammifères,*" of which seventy numbers have been published, was undertaken in conjunction with Geoffroy St. Hilaire, and is the most considerable and most extensive publication on Zoology which has appeared since the time of Buffon. He was likewise the author of many other works and memoirs on zoological subjects in various scientific journals and collections.

M. F. Cuvier, like his celebrated relative, combined a remarkable dignity and elevation of character, with the most affectionate temper and disposition. Like him, too, his acquisitions were not confined to his professional pursuits, but comprehended a very extensive range of literature and science. In his capacity of inspector of the university, he devoted himself with extraordinary zeal to the improvement of the national education of France in all its departments, from the highest to the lowest. It was in the course of one of his tours of inspection that he was attacked at Strasburg with paralysis; the same disease which, under similar circumstances, had proved fatal to his brother, and likewise in the same year of his age.

Dr. Martin van Marum was secretary to the Batavian Society of Sciences at Haarlem, and superintended the publication of their Transactions for many years. He was also director of the Teylerian Museum at the same place, and the noble library of natural history and science which adorns that establishment was chiefly collected by his exertions: it was under his directions also that the great electrical machine belonging to the Teylerian Museum was constructed, and he published in 1795 and 1800 the results of a

very extensive series of experiments on the various forms of electrical phenomena which were produced by it, and more particularly with reference to a comparison of its effects with those produced by a powerful voltaic pile, which were undertaken at the express request of Volta himself. Dr. van Marum was remarkable for his very various acquirements, and was the author of many memoirs in the Haarlem and other Transactions, on botanical, chemical, physical, and other subjects: he was a man of the most simple habits and of the most amiable character, and devoted himself most zealously during the greatest part of a very long life to the cultivation of science, and to the promotion of the interests of the establishment over which he presided.

Gentlemen, I have now arrived at the last and most painful part of my duty in addressing you, which is most gratefully and most respectfully to bid you farewell.

ASTRONOMICAL SOCIETY.

Nov. 9, 1838.—The following communications were read:—

I. Astronomical Observations made at the Imperial Observatory at Wilna, in the year 1835. By M. Slavinski.

These observations are of a similar nature to those made in former years, and communicated to the Society from time to time. The present collection consists of observations of *Jupiter*, *Saturn*, *Mars*, and *Uranus*, as well as of moon-culminating stars, occultations of stars by the moon, and eclipses of *Jupiter's* satellites. The geocentric right ascension and declination of each planet, and for each day of observation, are compared with the positions deduced from Encke's *Berlin Ephemeris*, and the differences noted. In the case of *Jupiter* these differences in right ascension are all positive, and the maximum difference is $1^s,36$; in declination the maximum differences are $-4''\cdot1$ and $+6''\cdot9$. In the case of *Saturn*, the differences are likewise (with one slight exception) all positive, the maximum being $0^s,80$; the same remark applies to the declinations, the whole of the differences being positive, and varying from $15''\cdot6$ to $28''\cdot1$. In the case of *Mars*, the differences in right ascension vary from $-0^s,25$ to $+0^s,52$; and in declination from $-1''\cdot9$ to $+17''\cdot6$. In the case of *Uranus*, the differences in right ascension are very considerable, and all positive, varying from $3^s,07$ to $4^s,02$; but in declination the differences are not so great, being confined within $-4''\cdot9$ and $+3''\cdot9$. These large errors in the tables of some of the planets are confirmed by observations made at other observatories; and will doubtless, in time, lead to their correction.

The observations of the moon-culminating stars were made on thirty-two days at various parts of the year, each observation being made with the five wires of the transit. Only six of these observations were made of the second limb of the moon.

The occultations are six in number; one being of *Saturn* on the 27th of August; and the others of various stars. There are ten eclipses of *Jupiter's* satellites; viz. five of the first, and five of the second. To the whole is subjoined the monthly mean of the baro-

meter and thermometer during the year, with a statement of the prevailing wind, which appears to be north-west and south.

II. A letter from Professor Bessel to Sir J. Herschel, Bart., dated Königsberg, Oct. 23, 1838.

Esteemed Sir,—Having succeeded in obtaining a long-looked-for result, and presuming that it will interest so great and zealous an explorer of the heavens as yourself, I take the liberty of making a communication to you thereupon. Should you consider this communication of sufficient importance to lay before other friends of Astronomy, I not only have no objection, but request you to do so. With this view, I might have sent it to you through Mr. Baily; and I should have preferred this course, as it would have interfered less with the important affairs claiming your immediate attention on your return to England. But to you I can write in my own language, and thus secure my meaning from indistinctness.

After so many unsuccessful attempts to determine the parallax of a fixed star, I thought it worth while to try what might be accomplished by means of the accuracy which my great Fraunhofer heliometer gives to the observations. I undertook to make this investigation upon the star 61 *Cygni*, which, by reason of its great proper motion, is perhaps the best of all; which affords the advantage of being a double star, and on that account may be observed with greater accuracy; and which is so near the pole that, with the exception of a small part of the year, it can always be observed at night at a sufficient distance from the horizon. I began the comparisons of this star in September 1834, by measuring its distance from two small stars of the 11th magnitude, of which one precedes, and the other is to the northward. But I soon perceived that the atmosphere was seldom sufficiently favourable to allow of the observation of stars so small; and, therefore, I resolved to select brighter ones, although somewhat more distant. In the year 1835, researches on the length of the pendulum at Berlin took me away for three months from the observatory; and when I returned, Halley's Comet had made its appearance, and claimed all the clear nights. In 1836, I was too much occupied with the calculations of the measurement of a degree in this country, and with editing my work on the subject, to be able to prosecute the observations of a *Cygni* so uninterruptedly as was necessary, in my opinion, in order that they might afford an unequivocal result. But, in 1837 these obstacles were removed, and I thereupon resumed the hope that I should be led to the same result which Struve grounded upon his observations of *α Lyræ*, by similar observations of 61 *Cygni*.

I selected among the small stars which surround that double star, two between the 9th and 10th magnitudes; of which one (*a*) is nearly perpendicular to the line of direction of the double star; the other (*b*) nearly in this direction. I have measured with the heliometer the distances of these stars from the point which bisects the distance between the two stars of 61 *Cygni*; as I considered this kind of observation the most correct that could be obtained, I have commonly repeated the observations sixteen times every night.

When the atmosphere has been unusually unsteady, I have, however, made more numerous repetitions; although, by this, I fear the result has not attained that precision which it would have possessed by fewer observations on more favourable nights. This unsteadiness of the atmosphere is the great obstacle which attaches to all the more delicate astronomical observations. In an unfavourable climate we cannot avoid its prejudicial influence, unless by observing only on the finest nights; by which, however, it would become still more difficult to collect the number of observations necessary for an investigation. The places of both stars, referred to the middle point of the double star, are for the beginning of 1838.

	Distance.	Angle of Pos.
a	$461''\cdot617$	$201^\circ\ 29'\ 24''$
b	$706\ \cdot279$	$109\ \ 22\ \ 10$

As the instrument gives, at the same time, the distance and angle of position, I have always observed both. But the position circle is divided only into whole minutes; which, in the distance of the first star, have the value of $0''\cdot134$; in that of the second, $0''\cdot205$. Moreover, other causes exist which may render the observation of the angle of position less certain than that of the distances. I have, accordingly, considered the first of these as of less consequence in so delicate an investigation, and concentrated my attention, as far as I could, upon the latter.

The following tables [which will be found in the Monthly Notices of the Society, vol. iv. No. 17.] contain all my measures of distance, freed from the effects of refraction and aberration, and reduced to the beginning of 1838. In these reductions, the annual variations employed of both distances are $= +4''\cdot3915$ and $- 2''\cdot825$; which I have deduced (on the supposition that the stars a and b have no proper motions) from the mean motions of both stars of 61 *Cygni*, which M. Argelander had lately found by comparison of my determination (from Bradley's observations) for 1755, with his own for 1830. In the meantime, we cannot regard these variations of distance as the *true variations;* because the stars compared may have proper motions, and, also, because it is not known whether the mean of the motions of both stars of 61 *Cygni* appertains to its centre, and whether *this* (motion) is proportional to the time. In what follows, let us denote the true variations of the distances by $+4''\cdot3915+\alpha'$ and $-2''\cdot825+\beta'$, the mean distances for the beginning of 1835 by α and β; the time, reckoned from this beginning, by t; the difference of the constants of the annual parallax of 61 *Cygni*, and of the comparison-stars a and b, by α'' and β''; and, lastly, the coefficients of the parallax depending on the place of the earth by a. Then the expressions of the distances at the beginning of 1838 are—

$$\text{For the star } a = \alpha + t\,\alpha' + a\,\alpha''$$
$$\text{For the star } b = \beta + t\beta' + a\,\beta''$$

These expressions, as they were at the time of each observation, I

have written against the observations; we can, therefore, by inspection, perceive how the observations agree with the theory.

If we compare both divisions of these tables, we shall perceive that the agreement of the observations with each other is considerably augmented by giving to α'' and β'' positive values; or, in other words, by admitting a sensible parallax. If we consider this parallax as vanishing, the sum of the squares of the remaining differences of the eighty-five observations of the star *a* can be diminished only to 4·4487; that of the ninety-eight observations of the star *b* to 4·7108. If, however, we determine α'' and β'', so that the observations may be represented as exactly as possible, we can reduce these sums to 1·4448 and 2·4469. By this means we obtain the mean error of an observation of the star $a = \pm 0''{\cdot}1327$, of the star $b = \pm 0''{\cdot}1605$. That the observations of the second star are less accurate than those of the first, I consider to be owing to the *difference* of the directions of the two stars with respect to the direction of the double star. The way in which I conceive this difference to affect the result I shall here leave unexplained; but refer to the complete discussion, which I shall enter into at some future time, of the parallax of 61 *Cygni*.

I have employed the preceding list of the observations of the distances of the star 61 *Cygni* from *a* and *b*, in two different ways, in order to deduce from it results for the annual parallax of *α Cygni*. I have first assumed α'' and β'' as independent of each other; or, in other words, considered it as not improbable that *a* and *b* themselves may possess sensible parallax. In this way I have found,

For the Star *a*.

		Mean Error.
Mean distance for the beginning of 1838	$461''{\cdot}6094$	
Annual variation $= +4''{\cdot}3915 - 0''{\cdot}0543$	$+4\ {\cdot}3372$	$\pm 0''{\cdot}0398$
Difference of annual parallax of 61 and *a* ... $\alpha'' =$	$+0\ {\cdot}3690$	$\pm 0\ {\cdot}0283$

For the Star *b*.

Mean distance for the beginning of 1838	$706\ {\cdot}2909$	
Annual variation $= -2''{\cdot}825 + 0''{\cdot}2426$	$-2\ {\cdot}5824$	$\pm 0\ {\cdot}0434$
Difference of annual parallax of 61 and *b* ... $\beta'' =$	$+0\ {\cdot}2605$	$\pm 0\ {\cdot}0278$

The observations seem also to indicate, that the difference of the parallaxes of 61 and *b* is smaller than that of 61 and *a*; which must be the case, indeed, if *b* itself have a sensible parallax greater than *a*. The difference of the computed values of α'' and β'', in fact, exceeds the limits of the probable uncertainty of the observations; but it is to be observed that the probability of *equal* values of α'' and β'' is not so small that we should be inclined to consider the difference of the two as *proved* by the observations. Further observations will increase the weight of both results, and, at the same time, give more accurate values of the annual variations.

I have, therefore, deduced a second result from the observations, which rests on the supposition that the parallaxes of *a* and *b* are *insensible*; or that α'' and β'' are equal. For this purpose, since both series must now be brought into connexion with one another, it was

necessary to deduce the *weight* of the observations contained in the second series, the weight of those in the first series being taken as unit. I have found it $=0{\cdot}6889$; and hence the most probable value of the annual parallax of 61 *Cygni* $=0''{\cdot}3136$. On this hypothesis, I find the mean distances of both stars for the beginning of 1838, to be $461''{\cdot}6171$ and $706''{\cdot}2791$; and the corrections of the assumed values of the annual variations, $=-0''{\cdot}0293$ and $+0''{\cdot}2395$. The mean error of an observation of the kind of which I have assumed the weight as unit, is $\pm 0''{\cdot}1354$, and the mean error of the annual parallax of 61 *Cygni*, $=\pm 0''{\cdot}0202$.

This hypothesis manifestly represents the observations somewhat less correctly than the first calculation which was instituted; but what we lose in this respect is not sufficient to outweigh the decided preference due to this last calculation. We can form a judgment upon this point by the following lists of errors of the observations, which contain their comparisons with two formulæ; namely, that of the first calculation and the present hypothesis. I have also added a third column, which contains the errors that arise when we assume the parallaxes α'' and β'' in the first formula as vanishing. This column also shows immediately what differences were still to be explained by the annual parallax. It shows, in fact, that these differences are commonly positive or negative, accordingly as the coefficient of the annual parallax, which the foregoing tables give, is positive or negative. [The tables here referred to will also be found in the Monthly Notices of the Society, as before.]

As the mean error of the annual parallax of 61 *Cygni* ($=0''{\cdot}3136$) is only $\pm 0''{\cdot}0202$, and consequently not $\frac{1}{15}$ of its value computed; and as these comparisons show that the progress of the influence of the parallax, which the observations indicate, follows the theory as nearly as can be expected considering its smallness, we can no longer doubt that this parallax is sensible. Assuming it $0''{\cdot}3136$, we find the distance of the star 61 *Cygni* from the sun 657700 mean distances of the earth from the sun: light employs $10{\cdot}3$ years to traverse this distance. As the annual proper motion of *a Cygni* amounts to $5''{\cdot}123$ of a great circle, the *relative* motion of this star and the sun must be considerably more than sixteen semidiameters of the earth's orbit, and the star must have a constant aberration of more than $52''$. When we shall have succeeded in determining the elements of the motion of both the stars forming the double star, round their common centre of gravity, we shall be able also to determine the sum of their masses. I have attentively considered the preceding observations of the relative positions; but I consider them as yet very inadequate to afford the elements of the orbit. I consider them sufficient only to show that the annual angular motion is somewhere about $\frac{2}{3}$ of a degree; and that the distance, at the beginning of this century, had a mininum of about $15''$. We are enabled hence to conclude that the time of a revolution is more than 540 years, and that the semi-major axis of the orbit is seen under an angle of more than $15''$. If, however, we proceed from these numbers, which are merely *limits*, we find the sum of the masses of both

stars less than half the sun's mass. But this point, which is deserving of attention, cannot be established until the observations shall be sufficient to determine the elements accurately. When long-continued observations of the places which the double star occupies amongst the small stars which surround it, shall have led to the knowledge of its centre of gravity, we shall be enabled to determine the two masses separately. But we cannot anticipate the time of these further researches.

I have here troubled you with many particulars; but I trust it is not necessary to offer any excuse for this, since a correct opinion as to whether the investigation of the parallax of 61 *Cygni* has already led to an approximate result, or must still be carried further before this can be affirmed of them, can only be formed from the knowledge of those particulars. Had I merely communicated to you the result, I could not have expected that you would attribute to it that certainty which, according to my own judgment, it possesses. I have the honour to be, esteemed Sir, yours, F. W. BESSEL.

XIV. *Intelligence and Miscellaneous Articles.*

SILICATES OF SODA.

M. FRITZSCHE has described two crystallized compounds of silicate of soda and water in a memoir read before the Imperial Academy of St. Petersburgh.

When silica is dissolved to saturation in a solution of caustic soda, a liquid is obtained, which is capable of being almost entirely converted into crystals. If the solution be concentrated, a crystalline mass is formed in a few days; whereas if it be moderately diluted, it deposits crystalline radiated hemispherical masses, or scales or laminæ composed of crystals which are more or less distinct. When this compound is prepared on a large scale, crystals are obtained of the size of a pea, which are perfectly formed, and have polished surfaces; these are the crystals which were submitted to analysis, and were used for the determination of the crystalline form; this salt was analysed in the usual way; that is, it was decomposed by hydrochloric acid, and the quantities of chloride of sodium and silica determined; 66·8 parts of the crystals, pulverized and pressed between folds of paper to remove their moisture as much as possible, gave 14·4 of silica and 27·5 of chloride of sodium, which is equivalent to 14·6 of soda; the quantity of water is then 37·8 or 56·59 per cent. On exposing some entire crystals, but which probably retained a little moisture, to a strong heat, the loss amounted to 57·23 per cent. The salt may therefore be considered as composed of

	By analysis.	Theory.
Silica	21·55	21·52
Soda	21·86	21·86
Water	56·59	56·62
	100·	100·

The author represents it by $Na^3\ Si^2 + 27\underline{\dot{H}}$.

If this salt be exposed to atmospheric air, it is altered by the absorption of carbonic acid, but it does not deliquesce. Placed under a receiver over sulphuric acid, it expands, first at the surface, and after some time even to the centre of the crystal, but it preserves its form. When heated to 122° Fahr. it fuses and forms a sirupy liquid, which on cooling does not solidify, but long retains its liquid state.

The form of these crystals has been ascertained by M. Nordenskiold; they belong to the prismatic system.

M. Fritzche has succeeded in obtaining another hydrate of the above silicate of soda, but he has not been able to ascertain the circumstances requisite for its production; it was procured in preparing the foregoing in large quantity; in spherical masses, the surfaces of which were covered with crystals; the crystals, though of determinate form, could not be measured, but belonged to the system of axinite; the properties of the salt were not determined with precision, owing to the small quantity obtained; this consisted of

	By analysis.	Theory.
Silica	26·20	26·94
Soda	26·80	26·53
Water	47·00	46·53
	100·	100·

The symbol is $Na^3\ Si^2 + 18\,\dot{\underline{H}}$.—*L'Institut*, No. 254.

[Representing silica by 16, soda by 32, and water by 9; the first salt may be regarded as a disilicate of soda with 10 eqs. of water; and the second as containing 6 eqs. of water.—R. P.]

RESPIRATION OF PLANTS.

M. Colin has read before the Academy of Sciences, a memoir on the respiration of plants, the experiments detailed in which were performed with M. Edwards, Sen.

Scarcely any of the phænomena of the respiration of plants have been hitherto recognised, except the disengagement of carbonic acid gas; and this has been explained by the combination of the oxygen of the air with the carbon of the grain. Thus, according to this theory, the grain is only acted upon by the atmosphere, and the action of water on the respiration of plants is not considered. In the respiration of leaves, carbonic acid is evolved during the night, and during the day it is absorbed, and oxygen is disengaged by the direct solar rays; and these facts are explained on the supposition that the carbonic acid absorbed is decomposed by the plant, its carbon appropriated, and the oxygen disengaged. But this explanation supposes the plant to possess a decomposing power which to MM. Edwards and Colin it seems difficult to admit; and they have in consequence resumed the examination of this function of plants.

Hitherto the experiments performed on the respiration of grain have always been performed in the air; or when they have been performed in water, the explanations of the phænomena have been

limited by what occurs in the air; what has been disengaged in the fluid has not been examined; but this has been done by MM. Edwards and Colin.

They took a globe with a straight neck, the capacity of which was from three to four litres of water, (about 183 to 244 cubic inches,) with which it was filled, and they then introduced forty large and perfect Windsor beans (*fèves de marais*). To the globe a bent tube was adapted, which was filled with water, and which terminated in a jar also filled with water. The beans were thus in contact only with the water and the air which it contained, and which could not be renewed on account of the mode in which the experiment was performed; and this is an important circumstance, and upon which the success of the experiment depends.

The first phænomenon which appeared was the disengagement of bubbles of air arising from the seeds; at the end of twenty-four hours the disengagement was considerable. At the expiration of four days the beans were weighed; they had increased twenty per cent. in weight; when put into the ground they came up perfectly, which proves that they had suffered no change. As to the production of gas, that which was disengaged after passing through the water, and received in the tube and jar, was only a sign of the function; it could be only that portion which the water did not dissolve, as it was gradually formed; it was therefore smaller in quantity than that which was dissolved. The quantity of air which had passed through the water without being dissolved amounted to from twenty to forty millimetres (1·22 to 2·44 cubic inches); but that which was dissolved in the water, and which was expelled from it by ebullition, was very considerable. Before the experiment the water in the globe contained about 4·577 cubic inches of air, and after the experiment more than 30·5 cubic inches of gas were expelled. Thus the action of the beans alone produced nearly 30 cubic inches of gas. No doubt, therefore, can exist as to the action of water in the respiration of the beans.

It was found that the gas generated consisted of, 1st, an enormous quantity of carbonic acid; 2nd, an almost infinitely small portion of oxygen; and 3rd, a very small quantity of a gas which appeared to be azote, or at any rate the authors at present so consider it; its proportion was rather smaller than that of the air contained in the water.

These experiments then prove that during the respiration of plants water is decomposed, and that the carbonic acid formed is derived from the oxygen of the water, which unites with the carbon of the grain. MM. Edwards and Colin propose to examine on a future occasion whether the carbonic acid thus formed is totally or partially disengaged, and whether the hydrogen of the water is absorbed by the grain.—*L'Institut*, No. 257.

VALERIANIC ÆTHER. BY MM. GROTE AND OTTO.

When valerianic acid or a valerianate is distilled with alcohol and sulphuric acid, a liquor is obtained which contains a large quantity of valerianic æther; this æther separates partly of itself and partly

by the addition of water. It has a penetrating odour of fruit and of valerian; it is colourless; its specific gravity is 0·894; its boiling point is 272° Fahr.; it is scarcely soluble in water, but dissolves very readily in alcohol, æther, and oils.

The results of the analysis with oxide of copper were

Hydrogen	10·736	10·851
Carbon	64·723	64·963
Oxygen	24·541	24·186
	100·	100·

Which indicate the annexed theoretical composition,

28 eqs.	hydrogen	174·713	10·623
14	carbon	1070·090	65·056
4	oxygen	400·000	24·321
		1644·803	100.

According to these results, this æther is formed of

1 eq.	valerianic acid	10 C+18 H+3 O
1	æther	4 C+10 H+1 O
		14 C+28 H+4 O

The other properties of this æther are similar to those already known.—*Journal de Pharmacie*, vol. xxiv. p. 365.

ACTION OF SULPHATE OF AMMONIA ON GLASS. BY M. MARCHAND.

A mixture of hydrochlorate and nitrate of ammonia attacks glass very strongly, especially when it contains lead; and the same is the case with sulphate of ammonia. This salt, as is well known, if neutral, is converted by the action of heat into an acid salt, giving out ammonia; it may therefore be considered, under these circumstances, as an acid salt. If it be heated in a glass vessel, it begins to fuse at about 284° Fahr.; up to 536° it suffers no alteration, but at this temperature it gives out ammonia, and sulphuric acid and sulphite of ammonia sublime, and then it may be observed that the glass is strongly attacked. All the interior surface becomes dull, because combination takes place between the sulphuric acid and the potash of the glass, and temporarily, probably, between the ammonia and the silicic acid. Most commonly the vessel, which is frequently acted upon to the middle of the glass, breaks, and from the fissures there proceeds a sort of saline fixed white mass, difficultly soluble, and which is easily found by the blowpipe to be sulphate of potash. M. Marchand states, that he has also frequently observed that watch glasses containing lead, which he employs to dry substances *in vacuo* over sulphuric acid, are covered, after a certain time (about two to four weeks) with numerous cracks, and from which he has easily detached small fragments. It was impossible, however, to determine that loss of weight occurred in this case: this phænomenon could not therefore be derived from a disengagement of inclosed air,

as Bischoff, who has made a similar observation, is inclined to suppose; the same appearances have not been observed on the receivers of the air-pump, nor in any other kind of glass.

Journal de Pharmacie, vol. xxiv. p. 367.

ACONITIC ACID.

M. Buchner, jun. has investigated the properties of this acid, which was first obtained by M. Peschier of Geneva in 1820, from the *Aconitum napellus* and *Aconitum paniculatum*. M. Reuss, having occasion to prepare a large quantity of the extract of aconite, obtained a considerable portion of a granular dirty white substance, consisting principally of aconite of lime, and this he sent to M. Buchner, who, in order to separate the aconitic acid from it, first washed it repeatedly with water and alcohol, to remove the extractive matter, and then dissolved it in dilute nitric acid. The solution being filtered, the aconitic acid was precipitated by excess of acetate of lead. The precipitate obtained, being well washed and separated by filtration, was diffused through distilled water, and submitted to a current of hydrosulphuric acid until the aconitate of lead was completely decomposed. The sulphuret of lead being separated, the liquor was evaporated with a gentle heat, and the aconitic acid formed a white granular crystalline mass. Some aconites are so rich in aconite of lime, that nearly as much may be obtained as of pure extract. In order to dry it sufficiently for analysis, the acid was dried in a current of air of the temperature of 248° Fahr. By combustion with oxide of copper the results obtained were,

Hydrogen	3·44	3·80
Carbon	41·01	41·84
Oxygen	55·55	54·36
	100·	100·

The composition of the acid as combined with oxide of silver, appeared to be

Hydrogen	2·23
Carbon	48·71
Oxygen	49·06
	100·

From which it appears that the free acid is composed of

4 atoms of hydrogen ...... =	24·959	3·42
4 ,, carbon.......... =	305·744	41·84
4 ,, oxygen .... =	400·000	54·74
$C^4 H^2 O^3 + H^2 O$ =	730·703	100·

while the combined acid consists of

2 atoms of hydrogen ...... =	12·479	2·02
4 " carbon.......... =	305·744	49·45
3 " oxygen .... =	300·000	48·53
$C^4 H^2 O^3$ =	618·223	100·

These experiments seem to prove that the acid contained in aconite has the same composition as maleic and fumaric acid; that is to say, in its free state, like them, the composition is the same as that of malic acid combined with bases, that it consists of equal atoms of hydrogen, carbon, and oxygen, and is distinguished from malic acid in a state of combination by containing an atom less of water.

The author states the difference between the properties of the aconitic, maleic, and fumaric acids: aconitic acid does not, like malic acid, yield regular and distinct crystals; it assumes the state of a crystalline crust or of mammellated masses, formed of crystals so small that their form cannot be determined even by the microscope. When pure, aconitic acid is perfectly white, unalterable in the air, and very soluble in water at all temperatures, it dissolves also very well in alcohol and in æther.; by evaporation it separates from its solutions in thick crusts, or in fine ramifications. In this respect it differs from fumaric acid, which is very slightly soluble, and from maleic acid, which crystallizes in very distinct prisms. As to its flavour, the paramalleic acid has a disagreeable nauseous taste, which aconitic acid does not possess. When exposed to heat these acids act in a very different manner, and may be readily distinguished; fumaric acid fuses with difficulty; when heated to above 392° Fahr., it is completely volatilized, and yields a solid sublimate. Maleic acid fuses at 248°, distils perfectly, and if the heat be raised a little, and the operation prolonged, it is converted into fumaric acid; when, however, it is quickly heated in a retort it is volatilized in a liquid state, yielding a little fumaric acid, and on cooling, the liquid product crystallizes.

Aconitic acid acts differently when heated on a sand bath in a long tube to 266° Fahr.; it becomes brown, but does not fuse. At this heat it begins to melt, but it does not boil till heated to 320° Fahr., and then it becomes red-brown. If it be kept for some time in this state, it is not converted into fumaric acid, but small colourless drops are observed to condense on the surface of the brown liquid, which on cooling become colourless prismatic crystals; but the greater part of this acid is converted into a brown tenacious hygrometric matter, which dissolves in water and does not crystallize.

Aconitic acid subjected to a sudden heat in a small much-inclined retort, fuses, becomes brown, yields white vapours which condense into a bright yellow liquid, of an acrid taste and empyreumatic smell. Charcoal remains in the retort, but the bright yellow liquid becomes full of slender prismatic crystals; when these are separated more crystallize in the mother water. These crystals are not aconitic acid; when precipitated by acetate of lead they give a flocculent precipitate, which differs from aconitate of lead by its greater solubility; the precipitate resembles those obtained with the maleic and malic acids, in being converted into brilliant crystals by contact with water, which is not the case with aconitate of lead.

Journal de Pharmacie, vol. xxiv. p. 403.

SEPARATION OF COPPER FROM ARSENIC.

According to M. Rose, the separation of these metals is to be effected by decomposing a solution of them by ammonia, and then digesting it for a considerable time in an excess of hydrosulphuret of ammonia, adding to the filtered solution either acetic or muriatic acid, which precipitates the arsenic in the state of a sulphuret.

M. Buchner having occasion to examine a case of poisoning by Scheele's green, resorted to the above method; but soon found that the sulphuret of arsenic so obtained contained a notable quantity of copper, notwithstanding the hydrosulphuret of ammonia had been previously shaken up with a portion of sulphur. The precipitate, on the addition of acetic acid, was of a dirty flesh-red colour.

M. Buchner after several trials effected the separation in the following manner:—

The metals were perfectly precipitated from a solution in muriatic acid by sulphuretted hydrogen. The precipitate, after being washed with water holding sulphuretted hydrogen in solution, was dried and weighed.

A portion of this precipitate was mixed with exactly four times its weight of carbonate of potash, and eight times its weight of nitre, and heated in a glass vessel until it melted. The resulting dark bluish gray mass was, on cooling, boiled for some time in a quantity of water. Pure oxide of copper remained undissolved, which was collected and weighed. The arsenic may be separated from the solution, and the quantity ascertained in the usual manner. A shorter process however is, after the separation of the oxide of copper, to supersaturate the solution by muriatic acid, and ascertain the quantity of sulphur or sulphuric acid by means of barytes. The quantity of sulphur added to the metallic copper, and that deducted from the weight of the combined sulphurets, gives the quantity of the sulphuret of arsenic, from which the weight of the arsenic may be calculated.—*Annalen der Physik und Chemie.* J. C. Poggendorff, vol. xliv. p. 137, 1838.

ON A METHOD OF DISTINGUISHING STRONTIAN FROM BARYTES AND LIME. BY HENRY ROSE.

The qualitative examination for strontian is so far difficult as all the substances which precipitate solutions of strontian, also more or less precipitate solutions of barytes and lime. In all cases with respect to reagents, strontian stands between barytes and lime. Some reagents, viz. sulphuric acid, solution of chromate of potash, benzoate of ammonia, iodate of soda, precipitate barytes perfectly from its solution; strontian less perfectly, and lime still less so; other reagents, as the alkaline oxalates, on the contrary, precipitate lime perfectly, strontian imperfectly, and barytes still more imperfectly. Some reagents which do not precipitate strontian or lime perfectly, precipitate barytes, viz. hydrofluosilicic acid; and there are others which can with certainty distinguish solutions of barytes or strontian from solutions of lime; as, for instance, solution of sulphate of

lime. But there is no known reagent which acts similarly towards solution of barytes and lime, and differently towards solutions of strontian. Ferrocyanate of potash is, however, such a reagent; this occasions precipitates in neutral solutions of barytes and lime, provided they are not too dilute; they are, according to Mosander and Duflos, combinations of ferrocyanate of potash and barytes, and of ferrocyanate of potash and lime. Even in diluted solutions of barytes this compound after some time crystallizes on the sides of the glass in very distinct crystals. Bunsen has described them in Poggendorff's *Ann.* vol. xxxvi. Strontian is not precipitated from solutions even when concentrated, nor are any crystals formed after resting for a long time. It appears, therefore, that ferrocyanate of potash in combination with strontian forms a soluble salt. By this means a neutral solution of strontian may be distinguished from one of barytes, and from lime, and also from magnesia, as the latter likewise gives a precipitate with ferrocyanate of potash.—*Poggendorff's Annalen*. 1838. No. 7. p. 445.

British Association for the Advancement of Science: Instruments for the Alleviation of Deafness.

We are requested to intimate, that a Committee having been appointed at the Newcastle meeting of the British Association, held in the month of August last, for the purpose of considering and reporting on the instruments which may be deemed best adapted for assisting the hearing in cases of deafness, the Committee will be happy in being favoured with the co-operation of such persons as may be disposed to assist their inquiries, either by suggestions, or by the loan of instruments or apparatus in explanation of their views.

Letters or parcels are requested to be transmitted, postage or carriage paid, to the care of Messrs. Taylor, Red Lion Court, Fleet Street, Printers to the Association.

METEOROLOGICAL OBSERVATIONS FOR NOVEMBER 1838.

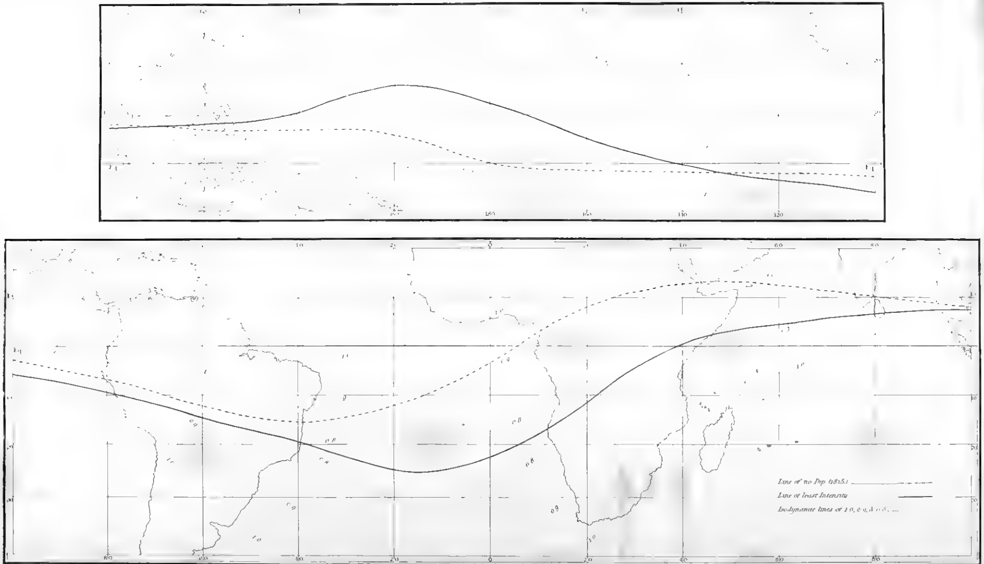
Chiswick.—Nov. 1. Overcast: rain: clear at night. 2, 3. Fine. 4. Rain. 5. Fine. 6. Very fine. 7. Rain: fine: windy at night. 8. Clear and fine: rain. 9. Heavy rain. 10. Clear and fine. 11. Dense fog. 12. Clear and cold. 13. Frosty: fine. 14. Frosty and foggy. 15, 16. Foggy. 17. Foggy: fine. 18, 19. Rain. 20. Cold haze. 21—23. Foggy. 24. Bleak and cold. 25, 26. Frosty. 27. Overcast. 28. Heavy rain: hurricane at night. 29. Boisterous with heavy rain: much thunder and lightning at night. 30. Rain. fine.

Boston.—Nov. 1. Fine: rain early A.M. 2, 3. Fine: rain P.M. 4. Cloudy: rain P.M. 5. Cloudy. 6. Fine. 7. Cloudy: rain early A.M. 8. Fine. 9, 10. Cloudy. 11. Foggy. 12, 13. Fine. 14. Foggy. 15. Cloudy. 16, 17. Foggy. 18. Cloudy: rain A.M. and P.M. 19. Stormy. 20, 21. Cloudy. 22. Cloudy: rain early A.M. 23, 24. Cloudy. 25, 26. Fine. 27. Stormy. 28. Cloudy: stormy with rain P.M. 29. Stormy: rain early A.M. 30. Stormy.

Applegarth Manse, Dumfries-shire.—Nov. 1. Heavy showers: hail. 2. Fair but cloudy. 3. Frequent showers. 4. Fair and cloudy. 5. Moist: slight showers. 6. Fair: one slight shower. 7. Rain all day: high flood. 8. Occasional showers. 9. Fine day. 10. Fine day: rain P.M. 11. Fine day after snow. 12. Hard frost: clear and serene. 13. Temperate. 14. Cloudy and raw. 15. Thick fog. 16. Cleared up: dry. 17. Rain in the night: cold. 18. Cold drying day: snow on hills. 19. Cold and threatening a fall. 20. Still cold, yet dry. 21. Still threatening a fall. 22—26. Cold and dry. 27. Cold: snow three inches deep. 28. Wet in the night: ditto P.M. 29. Very wet day and stormy. 30. Showery and stormy: flood.

Meteorological Observations made at the Apartments of the Royal Society by the Assistant Secretary, Mr. Roberton; *by* Mr. Thompson *at the Garden of the Horticultural Society at Chiswick, near London; by* Mr. Veall *at Boston, and by* Mr. Dunbar *at Applegarth Manse, Dumfries-shire.*

Days of Month. 1838. Nov.	Barometer. London: Roy. Soc. 9 a.m.	Barometer. Chiswick. Max.	Barometer. Chiswick. Min.	Barometer. Boston 8¾ a.m.	Barometer. Dumfries-shire. 9 a.m.	Barometer. Dumfries-shire. 9 p.m.	Thermometer. London: Roy. Soc. Fahr. 9 a.m.	Thermometer. London: Roy. Soc. Self-register. Max.	Thermometer. London: Roy. Soc. Self-register. Min.	Thermometer. Chiswick. Max.	Thermometer. Chiswick. Min.	Thermometer. Boston. 8¾ a.m.	Thermometer. Dumfries-shire. 9 a.m.	Thermometer. Dumfries-shire. 9 p.m.	Wind. London: Roy. Soc. 9 a.m.	Wind. Chiswic 1 p.m.	Wind. Bost.	Wind. Dumfries-shire.	Rain. London: Roy. Soc. 9 a.m.	Rain. Chiswick.	Rain. Boston.	Rain. Dumfries-shire.	Dew-point. Lond.: Roy. Soc. 9 a.m.
1.	29·420	29·428	29·371	28·96	29·30	29·24	48·7	49·2	39·6	52	36	45·0	40	36	w.	sw.	calm	se.	·383	·12	·15	...	42
○ 2.	29·358	29·424	29·316	28·92	29·12	29·41	41·7	51·0	38·7	49	30	42	38	34	s.	sw.	calm	sw.	·077	·04	...	...	40
3.	29·464	29·460	29·064	29·11	29·39	29·02	39·8	48·2	36·4	52	40	37	31	40	ssw.	s.	calm	s.	·038	...	·11	...	38
4.	28·848	29·034	28·844	28·45	28·89	29·05	45·6	47·9	40·6	56	39	45	42	44	s.	sw.	calm	w.	·133	·06	·15	...	41
5.	29·156	29·603	29·204	28·75	29·33	29·56	46·4	51·2	40·7	52	29	46	42½	36	sw.	w.	calm	w.	·050	...	·03	1·95	42
6.	29·722	29·738	29·687	29·28	29·70	29·61	40·4	50·3	38·9	56	42	39	30	36½	w.	sw.	calm	sw.	...	·26	...	...	38
7.	29·462	29·493	29·448	29·53	29·27	29·00	53·7	54·4	40·9	61	47	53	44½	46½	se. var.	sw.	calm	sw.	·166	·02	·18	...	45
8.	29·554	29·656	29·457	29·	29·12	29·26	50·4	57·7	49·3	57	44	48·5	46	44½	sw. var.	sw.	nw.	sw.	·011	·30	...	...	46
☾ 9.	29·370	29·590	29·374	28·97	29·27	29·40	46·3	54·7	46·3	52	32	44	42½	40	wnw.	sw.	calm	sw.	·250	·10	...	...	45
10.	29·708	29·826	29·720	29·25	29·70	29·70	43·8	50·3	39·5	50	28	43	38	31½	sw.	sw.	calm	w.	·061	...	...	...	42
11.	29·654	29·830	29·635	29·27	29·59	29·98	33·4	48·0	33·6	43	34	32	36	31½	w.	ne.	calm	e.	...	...	...	...	36
12.	30·096	30·389	30·126	29·80	30·28	30·34	37·9	41·6	37·3	47	27	36·5	30	36	n.	ne.	calm	e.	...	...	...	0·20	35
13.	30·424	30·462	30·125	30·03	30·40	30·36	37·9	46·6	35·7	48	25	31	38½	36	n.	n e.	calm	sw.	...	...	...	...	36
14.	30·250	30·264	30·064	29·98	30·28	30·12	37·8	47·7	35·3	48	38	26	34½	38	n.	e.	calm	sw.	...	...	...	...	34
15.	29·924	29·989	29·804	29·55	29·94	29·72	42·0	46·7	38·2	45	41	40	32½	34	e.	e.	calm	se.	...	...	...	...	38
16.	29·698	29·737	29·686	29·30	29·52	29·59	43·7	45·2	41·8	46	35	41	36½	40½	e.	n.	calm	se.	...	...	...	...	41
● 17.	29·746	29·772	29·697	29·34	29·68	29·77	41·8	46·7	41·0	51	40	42	38	34	s.	n.	calm	ne.	...	·32	...	...	38
18.	29·632	29·680	29·640	29·33	29·86	29·86	43·7	46·7	42·0	46	39	43	30	32	n.	ne.	calm	ne.	·261	·40	...	...	40
19.	29·566	29·589	29·540	29·37	29·77	29·72	40·0	45·0	40·8	41	37	42	36½	32	ne.	ne.	ne.	e.	·327	·10	·15	0·20	39
20.	29·656	29·677	29·569	29·39	29·74	29·66	38·8	39·8	37·8	40	35	40·5	36	46	nne.	ne.	calm	e.	·116	...	...	...	36
21.	29·410	29·455	29·263	29·19	29·55	29·50	38·7	40·7	36·4	50	49	39	36	56½	ne.	e.	e.	e.	...	·13	...	...	36
22.	29·166	29·406	29·184	28·95	29·48	29·40	44·4	45·3	38·4	50	37	42	38	38	ne.	e.	e.	e.	·122	·02	·06	...	40
23.	29·514	29·602	29·510	29·23	29·60	29·66	43·7	47·5	42·7	51	36	41	40	36½	ne.	e.	calm	e.	·033	...	...	...	40
☽ 24.	29·568	29·858	29·581	29·33	29·77	29·94	38·8	44·3	37·6	40	23	38	39½	32½	nne.	ne.	ne.	e.	...	...	...	...	35
25.	29·994	30·036	29·988	29·68	30·04	29·96	33·3	39·6	32·4	39	26	34	32	36	n.	e.	calm	ne.	...	...	...	...	34
26.	29·880	29·916	29·851	29·59	29·86	29·76	32·7	37·8	32·5	39	27	34	34	30	e.	e.	ne.	e.	...	...	...	2·22	28
27.	29·490	29·469	29·207	29·27	29·44	29·24	33·8	34·3	30·0	47	34	31	31	33	ene.	e.	ne.	e.	...	·04	...	...	30
28.	29·040	29·031	28·717	28·75	28·98	28·31	41·8	42·5	33·8	53	46	41	36	46	e.	e.	calm	e.	·041	1·21	·04	...	35
29.	29·658	28·879	28·673	28·22	28·12	28·18	48·7	51·7	41·8	53	44	50	44	44½	s.	ssw.	sw.	sw.	·216	·43	·30	...	42
30.	28·958	29·320	29·030	28·43	28·50	28·94	47·6	52·0	46·4	53	43	48·5	45	46	se.	s.	w.	wsw.	·450	...	...	...	42
Mean.	29·580	29·653	29·479	29·20	29·31	29·50	41·9	46·8	38·9	48·9	36·03	40·5	36·9	37·6					Sum. 2·735	3·55	1·17	4·57	Mean. 38·5



MAGNETIC LINES OF NO DIP, AND OF LEAST INTENSITY.

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[THIRD SERIES.]

FEBRUARY 1839.

XV. *Comparison of the Magnetic Lines of no dip and of least intensity.* By Major Sabine, F.R.S.

[With a Map, Plate IV.]

To Richard Taylor, Esq.

Dear Sir,

In my report on the magnetic intensity of the earth, published in the last volume of the Reports of the British Association, it is remarked, that "every geographical meridian has a point of minimum (magnetic) intensity; if these points in different meridians were connected by a line, that line would separate the intensities of the northern from those of the southern magnetic hemisphere. It would be in some respects analogous to the line of no dip; but it would not be a line of *equal* intensity, as it would consist of intensities varying from unity to the lowest on the globe. Such a line traced on the map is found to differ very considerably in geographical position from the line of no dip."

It may be interesting to those of your readers who are magneticians to compare the positions of these two lines; namely, that of no dip, and that of least intensity. They are drawn on the accompanying map, which is divided into two portions, to prevent its being inconveniently long for the octavo size, one portion containing 200 degrees, and the other the remaining 160 degrees. The lines of *equal* intensity (isodynamics) of 1·0, 0·9, and 0·8 are drawn in faint dotted lines, and are taken from the general map in the Report referred to. The line of *least* intensity, now drawn for

the first time, is characterized by a strong unbroken line. It has obviously two points of minima, and two of maxima; those of minima being nearly in the middle of the two divisions of the map, one in each, and the points of maxima coinciding nearly with the meridians which separate the divisions.. The line of no dip is marked by the broken line. It is taken from Captain Duperrey's map, and rests principally upon observations made by that distinguished officer in the voyage of circumnavigation of the Coquille, undertaken by order of the French Government for that express object. His observations were at stations in the immediate vicinity of the line of no dip, and occasionally on the line itself. In assigning its position from the former class of observations Captain Duperrey has employed the well-known formula of M. Biot's hypothesis and has computed by it the geographical distance to be allowed for the small observed values of the dip. The facts of observation are thereby in some degree mixed up in their representation with theoretical assumptions; but the distances of the places of observation from the line to be determined by them are usually so small, that any error which the theory may have induced must be insensible on the scale on which the present map is drawn.

The epochs to which the lines of dip and intensity respectively correspond are very nearly the same: that of the line of no dip is 1825, and the line of least intensity rests on observations of which the greater part were made between 1817 and 1836.

It will be seen that these lines, which until lately were considered to be identical, and which by many are still supposed to be so, with at most merely local interruptions, are *systematically* distinct, and in one quarter of the globe in particular are separated by a space equivalent to 20 degrees of latitude, or 1200 geographical miles.

I remain, dear Sir, yours, &c.,

Tortington, Jan. 1, 1839. EDWARD SABINE.

XVI. *On the Evolution of Heat by Thermo-Electricity. By Mr.* F. WATKINS.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

THE excitement of electricity by the influence of temperature is evident by the production of the spark, and of the chemical, physiological, magnetical and mechanical effects or-

dinarily attributed to that agent; but I believe it has never yet been recorded that the phænomena of heat, as evolved by the metal employed in its transmission, have been observed as a result of this mode of excitation.

I have lately observed this fact with a delicate air electro-thermometer of the construction suggested by my friend Mr. W. S. Harris, and also with a Bréguet's metalline thermometer, arranged with M. De la Rive's contrivance for passing a current through its helix.

The indications afforded by both instruments were very distinct and satisfactory, and could not be ascribed to any direct conduction of heat, for I placed ice around the wires, which completed the circuit between the battery and the indicating instruments.

I employed a massive thermo-electric battery, consisting of 18 pairs of bismuth and antimony prismatic rods, four inches long, united consecutively, similar to the arrangement of M. Van der Voort, of Amsterdam.

When the electricity was excited by applying a hot iron heater near one extremity of the battery and ice at the opposite extremity, with the circuit complete through the air electro-thermometer, the heating effect on the fine platinum wire in the spherical air reservoir was immediately visible by the ascent of the coloured liquid up the fine glass tube communicating with it. The elevation of the liquid column was about 20°, which occupied a space on the scale of one inch.

When the Bréguet's thermometer was placed in the circuit, the index attached to the bottom of the compound metallic helix moved round 10° in the direction of the coils, the helix expanding by the elevation of temperature conferred by the passage of the thermo-electric current.

The elevation of temperature of the metals forming the electric circuit in both instruments, was always manifested when the circuit was completed, and remained constant, but on breaking the circuit the loss of heat was very apparent; therefore I have no reason to doubt that the heating effects so conspicuously shown, were due solely to the transmission of the thermo-electric current.

I have repeated the experiments many times with uniform success; and if you consider the fact of sufficient importance to be announced in your Magazine, this brief notice is at your disposal for that purpose.

I remain, Gentlemen,

Yours, &c.

5, Charing Cross, 24th Dec. 1838. FRANCIS WATKINS.

XVII. *Preliminary Notice of some Experiments on the Action of Acetone on the Bichloride of Platinum* (Platin Chlorid.). *By* William C. Zeise *of Copenhagen* *.

When a solution of bichloride of platinum in about $2\frac{1}{2}$ parts of acetone is distilled to the consistence of syrup, and the product once or twice redistilled, a mixture of several new combinations of the protochloride (chlorür) which is thereby formed is obtained. The rectified fluid contains a considerable quantity of muriatic acid, and at least one æthereal substance. It is very difficult to obtain these different combinations separate, and though I have spent already considerable time in the attempt, I am not even yet certain that I have obtained them all in a state of purity. One of these compounds, a yellow crystallizable substance, the composition of which may be thus represented,

$$\text{Pl.} + 2\,\text{Cl.} + 6\,\text{C.} + 10\,\text{H.} + \text{O.}$$

appears to deserve a particular description. To obtain this substance, the brown, tarry, acid residue is agitated with fresh portions of water so long as this acquires a brownish yellow colour, and the solution quickly filtered through linen to separate the undissolved resin or pitch-like matter. The lower part of the solution soon becomes turbid, and in the course of half an hour or an hour a tolerably large quantity of small yellow crystals precipitates. The mother liquor, when separated from the crystals, is placed, *in vacuo*, over sulphuric acid with hydrate of potassa or lime until it has evaporated to a brown crystalline mass: this mass is then to be treated with water in the same way as before, by which a new portion of the crystalline body will be obtained; but this is almost always of a dark brown colour. To obtain it pure it must be dissolved in the acid fluid obtained by distilling the solution of the bichloride in acetone, and the solution so made distilled to the consistence of syrup, and then treated with water in the manner before described. Finally, in order to remove all remaining traces of the brown colouring matter, the crystallized product, after being well pressed and dried between the folds of bibulous paper, is to be dissolved in acetone, the saturated hot solution filtered into a wide-mouthed glass vessel, and, when cooled, and the crystals which have been deposited separated, it must be evaporated by careful distillation to that point, when on cooling the greatest part of the substance is deposited. The crystals thus obtained are to be washed in a small quantity of acetone, and then dried.

* Translated and communicated by E. Solly, Jun.

I have likewise obtained a considerable quantity of this substance by keeping, for 24 hours, a mixture of acetone with the bichloride in a well-closed vessel. I call this substance *Metacechlorplatin*.

Metacechlorplatin is of a sulphur yellow colour; the crystals are small and difficult to measure. It is very nearly without any smell. After having been dried in the air, it loses nothing in weight when placed in a vacuum over sulphuric acid, whether at common temperatures or raised above 100°. It inflames very easily, and burns with a partially green flame, leaving a silvery white residue of platinum. Heated in a retort it blackens, and without swelling up it gives out abundance of vapours, which are at first peculiar, but afterwards smell strongly of muriatic acid; a part, at least, of these vapours easily condenses into an oily substance. The carbonaceous residue burns slowly in the open air, like tinder, and leaves a residue of silvery white platinum. At common temperatures it is almost wholly insoluble in water; when heated in it, it forms a yellow solution, which, however, contains only a very little of the salt. When boiled, this solution threw down a brown flocculent substance, whilst that portion which was not soluble was changed into a brown, slimy mass without any visible appearances of metallic platinum. It seems to be quite insoluble in æther. At common temperatures alcohol acts but little upon it, but when heated it dissolves a portion, acquiring at the same time a yellow colour, on cooling a yellow crystalline powder is deposited. Muriatic acid, even the most concentrated, dissolves it only at higher temperatures; the acid solution may be raised to the boiling point without visible change. A solution of caustic potassa easily dissolves metacechlorplatin, forming a brown solution. Solutions of chloride of potassium or sodium, when heated, also dissolved it, and the yellow solution underwent no change on boiling. The determination of the quantities of carbon and hydrogen in metacechlorplatin was made by burning one portion with oxide of copper, and another portion with chromate of lead. Its composition ($Pl.Cl^2 + C^6 H^{10} O$) shows by comparing it with that of acetone, ($C^6 H^{12} O^2$), that in it one atom of the protochloride of platinum replaces $H^2 O$. As however there appears to be formed at the same time several other combinations of chlorine, the action is probably not so simple. Certainly it is probable that a combination takes place of 2 atoms of chlorine with 2 atoms of hydrogen as in the action of bichloride of platinum on alcohol, and in this case there is formed likewise through the action of 1 atom of oxygen, one of the compounds resembling aldehyde (see my paper on the

inflammable chloride of platinum in Poggendorff's *Annalen*, vol. xl. p. 251.). It also appears worthy of notice that, whilst by the action of bichloride of platinum on alcohol, 2 atoms of the protochloride combine with 1 atom of ætherin ($C^4\ H^8$), here only 1 atom of the protochloride, but in addition to this also $H^2.\ O.$ (which perhaps replaces the second atom of the chloride) forms a combination with $C^6.\ H^8$.

After obtaining the last brown crystallized portions of metacechlorplatin by evaporation *in vacuo*, there remained still a brown sour fluid; if this were heated in a retort it became turbid and entered into tolerably strong effervescence, during which an ætherial fluid passed over; and in the course of from half an hour to one hour, large quantities of flocculi of a coal-black colour had separated from the fluid, which had then become colourless. I will here only remark of this body, that when slightly heated, it burns with explosion; for the present I shall it *pueracechlorplatin*.

When water can dissolve no more of the original syrupy product of the distilled solution of the bichloride, there remains a considerable quantity of a brownish black pitch-like substance—for convenience sake I call this *platinharz**. At common temperatures it is brittle like resin, and breaks with a vitreous fracture; and when it is very carefully digested in water and then dried in a vacuum over sulphuric acid and hydrated potassa, it is very easily pulverizable. When slightly warmed it becomes soft, and may then be kneaded like wax and drawn out in threads. Inflamed it burns with a very brilliant and somewhat greenish flame, leaving metallic platinum. Heated in a retort it swells up considerably, giving off at the same time abundant fumes, of which a portion easily condenses; the carbonaceous residue burns in the air very slowly and leaves metallic platinum. Caustic potash dissolves the resin entirely; acetone almost the whole, and alcohol and æther the greater part of it. That portion which was insoluble in the two last-mentioned fluids dissolves in acetone, from which a brownish black body (soluble only in acetone and a solution of caustic potassa) was separated by æther: for the present I shall call this substance *chlorseplatin*. That portion which is soluble in alcohol and æther appears however still to contain two distinct bodies. This, like the metacechlorplatin, as well as the other primary and secondary products, I hope soon to be able to describe more fully. In connexion with this, I am at present also occupied with investigating the action of metaceton, pyroligneous spirit, and oil of turpen-

* Platinum Resin. E. Solly.

tine upon the bichloride of platinum. I also intend to examine some other metallic chlorides and haloid bodies under the same circumstances.

XVIII. *On the Composition of certain Mineral Substances of Organic Origin. Nos.* VI. VII. VIII. *Mineral Resins.*

By JAMES F. W. JOHNSTON, *M.A., F.R.SS. L. & E., F.G.S., &c. &c., Prof. Chem. and Min., University, Durham.**

No. VI. *Highgate Resin or Fossil Copal.*

THIS substance derives its names from the locality in which it was first found in any quantity, the blue clay of Highgate Hill near London, and from its similarity to copal resin in hardness, colour, lustre, transparency, and difficult solubility in alcohol†. For the two fragments which have afforded me the following results I am indebted to the liberality of my friend Mr. Brooke.

1. The first fragment analysed was translucent, of a dirty gray colour, and when broken emitting a resinous odour. In the air it volatilized by a gentle heat, leaving a small residue of charcoal and earthy matter. The former being burned away, the latter weighed 0·136 per cent.

9·905 grs. burned with oxide of copper gave 10·508 grs. of water and 30·795 of carbonic acid. This is equal to

Carbon.........	=	85·408
Hydrogen......	=	11·787
Oxygen	=	2·669
Ashes	=	0·136—100·000.

2. The second fragment was clear, pale yellow, and semitransparent. It was covered with a thin coating of a browner apparently altered variety, of which, from the smallness of the quantity at my disposal, I was not able wholly to divest it.

5·509 grs. gave 5·69 grs. of water and 17·07 of carbonic acid. This is equal to

Carbon............	=	85·677
Hydrogen........	=	11·476
Oxygen	=	2·847—100·000.

Assuming the latter specimen to be the purer, this substance is represented by the formula $C_{40}\,H_{32}\,O_1$, since

			Calculated.	Found.
40 Carbon	=	3057·480	85·968	85·677
32 Hydrogen	=	399·347	11·228	11·476
1 Oxygen	=	100·000	2·804	2·847
		3556·827	100·000	100·000

* Communicated by the Author.

† For a description of this substance see Phillips's Mineralogy.

The small defect of carbon and excess of hydrogen in the analysis are both on the side in which they ought to appear when experimental results are compared with an accurate formula.

This resin therefore presents another example of the connexion of resinous substances with oil of turpentine C_{40} H_{32}; and if the rational formula C_{40} $H_{32}+O$ be the true one, it exhibits the lowest state of oxidation of this radical with which we are yet acquainted. It is very slightly acted on by alcohol, but the solution gives a white precipitate with an alcoholic solution of acetate of lead. It is either altogether an acid resin therefore, or it contains a small quantity of a more soluble resin which is so.

VII. *Resin from Settling Stones.*

In Brewster's Edinburgh Journal of Science, N.S., vol. iv. p. 122, I described as *a new variety of mineral resin*, a substance I had met with among the old heaps of a lead-mine in Northumberland known by the name of Settling Stones, the working of which has been recently resumed. This mine is situated at the point of junction of a number of intersecting faults and veins, along which the strata are thrown up and down in various directions. By one of these faults the great whin sill of that district is brought to day, and forms an escarpment over which the waters of a little rivulet descend from a height of 20 or 30 feet. Near the veins the trap is much impregnated with lime, and the cheeks or walls of the vein are sometimes almost a perfect limestone, and have a gray or bluish gray colour. It is on these walls of the vein, resting on and occasionally covered by calc spar, brown spar, or pearl spar, that the resinous substance occurs. It is in the form of drops or flattened portions, more or less rounded, as if it had once been in a fluid or softened state. It is hard, brittle under the hammer, but exceedingly difficult to reduce to fine powder in the mortar: even after long rubbing the angular fragments can still be recognised. Its colour varies from pale yellow to deep red, its specific gravity from 1·16 to 1·54, and it exhibits a pale green opalescence. It does not melt at 400° Fahr., but it burns in the flame of a candle, and gives empyreumatic products when fused in a close tube over a spirit-lamp. It is insoluble in water and is very slightly acted upon by alcohol.

Having a small quantity of this resinous substance at my disposal I availed myself of the opportunity of determining its composition, with the view of comparing it with that of the fossil copal above analysed. In external appearance they

possess some resemblance, but their origin must be considered as entirely different. The one occurs in a vast deposit of tertiary clay, the other in a mountain limestone district, and in the centre of an enormous intruding mass of stratiform basalt.

5·83 grs. gave 5·695 of water and 17·95 of carbonic acid: 1·29 grs. left on burning in the air 0·042 of brown ash = 3·256 per cent. These are equal to

Carbon ……	85·133	or 40 atoms.
Hydrogen…	10·853	or 31·2 atoms.
Ashes ……	3·256—99·242.	

It is therefore doubtful whether this resinoid substance really contains any oxygen or not. It may be only an impure carbohydrogen = $C_4 H_3$, agreeing in composition with the hypothetical acetyle, or it may, like petroline, contain the elements in the proportions in which they exist in oil of turpentine $C_{40} H_{32}$. I regret that my supply of the substance did not permit me to repeat the analysis; and though I have revisited the mine in the hopes of obtaining a fresh supply, I have not been fortunate enough to meet with a single specimen.

VIII. *Berengelite.*

The specimens of the substance for which I propose the name of Berengelite were given to me by my friend Mr. Fryer, of Whitley House, near North Shields, and were obtained by him during his residence in South America. Of the circumstances under which it occurs Mr. Fryer thus writes to me:

"Of the resin or asphaltum from South America, I can unfortunately give you but a very imperfect account. I one day found in the yard of the Custom House at Arica a large convoy of llamas loaded with it, and all the information I could obtain from the men having charge of it was that they brought it from the province of St. Juan de Berengela, about 100 miles from Arica, that it was found in very large quantities, and formed, according to their description, something like a lake resembling the pitch lake of Trinidad. It is extensively used for paying boats and vessels at Arica, and, I believe, on the whole coast of Peru."

This substance is hard, brittle, may be scratched by the nail, has a resinous fracture and lustre, is of a dark-brown colour with a tinge of green, but gives a yellow powder. The external appearance of the masses as they were brought home appears to indicate that the whole had formerly been in a softer state so as to yield easily to compression. It is insoluble in water, but dissolves readily and in large quantity in

cold alcohol or æther, giving brown solutions. A small residue of earthy impurities is left. By evaporating the alcoholic solution, the resin is obtained of greater transparency, transmitting light of a bright red colour, fusing easily on the water-bath, and remaining soft and unctuous at the ordinary temperature of the atmosphere. It gradually recovers its brittleness, but after the lapse of three or four months it is still soft, and adheres in some measure to the fingers. This property appears to be possessed by many other resinous substances and explains the semifused appearance of the imported masses.

It has a peculiar, unpleasant, resinous odour. After fusion for some time at 212° Fahr., the unpleasant odour disappears, and is succeeded by an agreeable fragrance. On cooling again, it resumes after some time its original smell. When chewed in the mouth it imparts a slight sensation of bitterness; but the alcoholic solution has a disagreeable very bitter taste.

Like most other resins it is nearly insoluble in a concentrated solution of caustic potash. Boiled in more dilute alkali it gives a yellow solution, from which the resin is again precipitated by acids. The alcoholic solution gives with a similar solution of acetate of lead a copious yellow precipitate. It is therefore an acid resin. Its alcoholic solution is rendered milky by liquid ammonia and passes milky through the filter.

Burned with oxide of copper

12·732 grs. gave 33·37 of carbonic acid and 10·54 of water.
12·40 grs. gave 32·44 of carbonic acid and 10·445 of water.

These are equivalent to

	1.	2.
Carbon	72·472	72·338
Hydrogen.........	9·198	9·359
Oxygen	18·330	18·303
	100·000	100·000

These results agree very nearly with the formula $C_{40}\ H_{31}\ O_8$, which gives

40 Carbon ...	=	3057·480	=	72·036
31 Hydrogen	=	386·8676	=	9·115
8 Oxygen ...	=	800·000	=	18·849
		4244·3476		100·000

and represents a constitution analogous to that of colophony and some other resins of which oil of turpentine is the radical. But as the quantity of carbon indicated by this formula is less than that found by analysis, we ought probably to prefer one or other of the two formulæ

$$C_{41}\,H_{31}\,O_8 = \begin{cases} \text{Carbon} & 72{\cdot}533 \\ \text{Hydrogen} & 8{\cdot}929 \\ \text{Oxygen} & 18{\cdot}538 \end{cases} \quad \text{or } C_{41}\,H_{32}\,O_8 = \begin{cases} \text{Carbon} & 72{\cdot}322 \\ \text{Hydrogen} & 9{\cdot}215 \\ \text{Oxygen} & 18{\cdot}463 \end{cases}$$

$$100{\cdot}000 \qquad\qquad 100{\cdot}000$$

I am inclined, therefore, in the mean time to prefer the former of these two $C_{41}\,H_{31}\,O_8$, though the discovery of any mixture or impurity in the resin may hereafter show either of the others to be a more correct representation of its elementary constitution. The establishment of the first, $C_{40}\,H_{31}\,O_8$, from its analogy with the formula for colophony, would appear from theory the most likely, were there not other resins, the analyses of which I have already published, which appear to deviate from the general formula

$$C_{40}H_{32-x}O_{x+y}$$

by which many of the resins may be represented. To this subject I shall return in a future paper.

Origin of the Mineral Resins.

As I am not in possession of any other mineral substance of organic origin exhibiting the characters of a resin, it may not be improper here to advert to the probable origin of this class of substances.

1. *Fossil Copal.*—The composition of this substance clearly indicates a vegetable origin. It has been found in small quantities disseminated through the London clay. Under what circumstances could this vast deposit of clay be formed? Most probably along the course, or in the estuary of some great river, or in the bottom of a lake into which its waters were poured. And if at this period the climate were warmer in these latitudes than it now is, a circumstance in regard to which geologists seem agreed, we should expect to find (recent) resins similar to the fossil copal in similar localities, if any now exist and under a similar sun. From what we know of the Guianas stretching between the river Orinoco on the north and that of the Amazons on the south, a country abounding in rivers and lakes, liable to heavy rains and floods with a climate hot and moist, we should suppose it to be not very unlike that which poured its muddy rivers into the London basin, and buried beneath its waters occasional fragments of its trees, its resins, and its other vegetable productions.

From the island of Cayenne on this coast, and probably from the interior of French Guiana, is imported a resin the produce of the locust tree, which like the Highgate resin has much resemblance to copal, and is known in commerce by the name of animé resin. This resin has been analysed by

Laurent, and found by him to have a composition approaching very nearly to that above given for the fossil copal. The comparative results are as follows:

Animé Resin.		Fossil Copal. 1.	2.
Carbon	84·6	85·408	85·677
Hydrogen ...	11·5	11·787	11·476
Oxygen ...	3·9	2·669	2·847
	100·0	99·864	100·

From the above result he deduces for animé resin the formula $C_{40} H_{33} O$, which gives per cent.

Carbon	85·66
Hydrogen	11·538
Oxygen	2·802
	100·

The numbers given by this formula differ from those obtained by Laurent in the oxygen and carbon to the amount of one per cent., while they are almost identical with those given by the analysis of the fossil copal. As the hydrogen however is always in excess, the formula $C_{40} H_{32} O$ above deduced from my analysis is to be preferred.

Without, however, dwelling upon this discrepancy, it is interesting to find a resin still growing nearly if not absolutely identical in constitution with one produced and buried at a period so remote; and while this fact establishes the vegetable origin of the fossil copal, it may be considered as throwing some additional light on the nature of the climate at that remote epoch and as confirming the evidence of other facts in regard to the temperature of these latitudes during the deposition of the London clay.

2. *Resin of Retinasphalt* * = $C_{21} H_{14} O_3$.

The origin of this substance, found in the tertiary formation of Bovey in Devonshire, of nearly the same age as the London clay, is clearly indicated by the mode in which it occurs. It is scattered throughout the deposits of lignite, and is penetrated by twigs and hollow quadrangular spines, apparently the leaves of a coniferous tree. That it is the resin of some such trees is therefore very probable; that it has flowed in a liquid state is also probable from its being mixed with so much clay, and it is not unlikely that it may have undergone some change of composition since it was first deposited. We know as yet however too little in regard to the composition

* See Lond. and Edinb. Phil. Mag., vol. xii. p. 560.

of the resins which exude from the pines of warm climates to justify us in attaching much weight to this last conjecture that it has undergone a change since it was deposited. Colophony is $C_{20}\,H_{15}\,O$, so that this resin *might* be formed by substituting an atom of carbonic acid for one of hydrogen, since

$$C_{20}\,H_{15}\,O - H + CO_2 = C_{21}\,H_{14}\,O_3$$

3. *Middletonite* * $= C_{20}\,H_{11}\,O$.

From the circumstances under which this substance occurs in the coal of Yorkshire and Staffordshire in thin layers and masses in the body of the coal, I have already stated that it is to be regarded as the *altered* resin of the trees of the epoch of the great coal formation. That it has undergone a change is evident not merely from its properties, but from the apparent impossibility that a resinous substance should remain unaltered while the wood which enveloped it was converted into coal. Mr. Embleton, the intelligent viewer of the Middleton coal mines, considers the opinion of its being a changed resin to be confirmed by the appearance of the coal with which it is in contact, which appears to him to bear a close resemblance to a *changed bark*.

The pseudo resin of Settling stones is probably no further of vegetable origin than that it may have been distilled or volatilized out of vegetable matters scattered throughout the dark shale and other rocks with which the trap, near which it is found, had come into contact while in a fluid state.

4. and 5. $\begin{cases} \textit{Guyaquillite}\dagger = C_{40}\,H_{26}\,O_6 \\ \textit{Berengelite} = C_{41}\,H_{31}\,O_8. \end{cases}$

With the geological position of these two substances I am wholly unacquainted. The one is said to form large deposits in the neighbourhood of Guyaquil, the other to occur at least 15 degrees further south, forming a species of lake. The proximity of the volcanic chain to both localities, the former being almost at the foot of Chimborazo, renders it not unlikely that they may be, or may have been, distilled from vegetable deposits lying beneath; and though true resins have seldom been met with except in substances of known vegetable origin, yet since the petroline of Boussingault contains carbon and hydrogen in the same proportions as in oil of turpentine, there is no difficulty in conceiving that under favourable circumstances this and other compounds of similar constitution, existing in deposits of petroleum, may undergo oxidation and produce resins similar to those actually found as mineral productions in South America. Before we can obtain clear ideas on the subject however, we must obtain more exact in-

* See L. and E. Phil. Mag., vol. xii. p. 261. † See vol. xiii. p. 329.

formation regarding the circumstances under which they actually occur.

The pastò varnish described by Boussingault* is closely allied in constitution to the berengelite. This substance when freed by digestion in alcohol from a little green resin with which it is mixed, is colourless and possesses many of the properties of caoutchouc. It is very tenacious, and stretches into thin membranes, which are applied as a varnish to wooden vessels. It adheres strongly and after a time hardens, but never cracks. It forms a soap with caustic potash, from which acetic acid separates it. When dried and heated to 130° Cent. = 266° Fahr., the varnish thus separated melts, and on cooling is brown, tenacious, and (*now*) soluble in all proportions in alcohol, æther, and oil of turpentine. It does not appear from Boussingault's paper whether the previous solution in caustic potash be necessary to the production of this change, though he does speak of a remarkable modification being caused by caustic potash. The composition of the pure varnish (A) and of the *resin* or modified varnish (B) are thus given, the first being a mean of three, the second of two analyses.

	A.	B.	Calculated.
Carbon =	71·766	71·25	71·825
Hydrogen =	9·633	10·30	9·381
Oxygen =	18·600	18·45	18·794
	99·999	100·000	100·000

The third column is calculated according to the formula $C_{40}\,H_{32}\,O_8$, which agrees very well with the analyses of the varnish. It is not improbable however that the *resin* should be represented by a formula somewhat different.

The pastò varnish is brought to the neighbourhood of Quito from the high land of Macao, on the eastern slope of the Andes, from which the waters descend to the river of the Amazons. Except that it is of vegetable origin nothing is known regarding it, the tribes of Indians from whom it is obtained being still independent. The difference of its properties previously to fusion forbid the supposition that it is identical with the berengelite; were a distance of 20 degrees of latitude between the places from which they are respectively brought, not sufficient to render a common origin highly improbable.

It may not be uninteresting to present here a comparative view of the formulæ by which the several resins of which analyses have hitherto been published may be represented.

* *An. de Chim. et de Physique*, vol. lvi. p. 216.

Resin from the root of *Arbrea Brai*	$= C_{36}\ H_{30}\ O$	Dumas.
Animé resin	$= C_{40}\ H_{33}\ O$	Laurent.
Fossil copal	$= C_{40}\ H_{32}\ O$	
Elemi resin	$= C_{40}\ H_{32}\ O_2$	Rose.
Colophony (pinic and sylvic acid)	$= C_{40}\ H_{30}\ O_4$	Liebig and Trommsdorf.
Pastò varnish	$= C_{40}\ H_{32}\ O_8$	Boussingault.
Berengelite	$= C_{40}\ H_{31}\ O_8$? or $= C_{41}\ H_{31}\ O_8$	
Middletonite	$= C_{40}\ H_{22}\ O_2$	
Guyaquillite	$= C_{40}\ H_{26}\ O_6$	
Resin of retinasphalt (retinic acid)	$= C_{40}\ H_{27}\ O_6$? or $= C_{42}\ H_{28}\ O_6$	
Gamboge	$= C_{40}\ H_{24}\ O_8$? or $= C_{48}\ H_{29}\ O_{10}$	
Eblanin	$= C_{40}\ H_{20}\ O_8$? $= C_{42}\ H_{18}\ O_8$	Apjohn and Gregory.

In these formulæ, which from the nature of the substances may all be subject to future corrections, we see a mode of representing, at least approximately, all the analysed resins (with one exception) by quantities in which that of carbon is constant. It would be a very beautiful general expression which in the form of $C_{40}\ H_{32-x}\ O_{x+y}$ should represent the constitution of all or of one great group of the resins. Our data, however, are not yet sufficiently extensive to enable us to form a decided opinion on the subject. In a future paper I shall give the composition of some other resins, and consider this point at greater length than it would be proper to do at the close of the present paper.

Durham, Dec. 6, 1838.

XIX. *Meteorological Observations during a Residence in Colombia between the Years* 1820 *and* 1830. *By* Colonel Richard Wright, *Governor of the Province of Loxa, Confidential Agent of the Republic of the Equator, &c. &c.*

[Continued from p. 18.]

ALTHOUGH it scarcely falls within the limits of a mere meteorological journal to expatiate on the wide field of inference which opens to our view when we reflect on the influence of temperature, not merely on animal but on social life, yet the operation of local circumstances has been so striking, and will probably play so important a part in the future destinies of the South American continent, that it is difficult to forbear some remarks on so interesting a subject.

Climate is one of the first agents which operates upon the propagation of the human race over the face of the globe, presenting itself sometimes as a benignant conductor, at other times raising a hostile barrier which science and industry slowly overcome. The Spaniards who peopled that part of South America now under consideration, as soon as they had formed on the coast the establishments necessary to preserve their connexion with the mother country, seem to have traversed hastily the fertile but insalubrious lowlands to meet on the Cordillera a temperature adapted to their habits and constitutions. The dominion of the Incas had, upon similar principles, extended itself along the immense ridge; and the descendants of the conquerors and conquered are, to this day, found united on the same elevations, from whence the population has descended gradually into the plains; and would have done so much more slowly, but for the importation of the African race, who find on the sandy coast and sultry savana a climate congenial to their constitution. It may be a matter of curiosity to inquire, why that portion of the *bronzed race* which constituted the empire of the Incas and of the Lipas has constantly exhibited a constitutional type so different from the tribes of the same race now thinly scattered through the plains and valleys. The dominion of the Incas could scarcely be said to have established itself in the lowlands. With the exception of the dry narrow tract of the Peruvian coast, their empire was exclusively of the mountains; and the Indians who speak the *Quichua*, or general language of the Incas, still manifest the same preference for cold and elevated situations; sleeping in the open air rather than under a roof, and exhibiting an insurmountable repugnance to descend into the hot country, where they fall victims more rapidly than even the Europeans. The latter, although commercial interests have led them to form establishments on the coasts, and more partially on the great rivers, may be said to live in a state of perpetual hostility with the climate. Their complexions become sallow, their frames feeble; and although, where heat is uncombined with great moisture, as in Cumanà, Coro and Maracaybo, they are subject to few diseases of a violent character, the strength is gradually undermined, and the species may be rather said to vegetate than to increase. The individuals of African race, who complain of cold when the yearly mean is 75°, alone develope all the physical strength and energy of their character in the hot lowlands of the coast and interior. The mixed race, or people of colour, unite to bodily hardihood intrepidity, ambition, and a deadly feeling of those prejudices which, in spite of laws, continue to separate them from the *white* descendants of

the Spaniards, who thus encounter, both in the high and lowlands, two races in whom the seeds of hostility have been sown by injustice, and fostered by mistaken feelings of interest and vanity*. It is on the mountain slopes of from 3000 to 7000 feet that we encounter climates most analogous to our ideas both of health and pleasure. Raised above the noxious miasmata of the coast, we dwell in perpetual summer amid the richest vegetable productions of nature, amid a continued succession of fruits and flowers. This picture however must not be considered as universally exact. In those unbroken forests where population has made little progress the sky is often clouded, and the soil deluged with continual rains. The western declivities of the Andes, which front the Pacific, are particularly exposed to this inconvenience.

It might be expected that with regard to human life and vigour, the elevated plains of the Andes would correspond to the northern countries of Europe. This however, as far as regards the inhabitants of European race, does not seem exactly to take place. It is true they escape the bilious and intermittent fevers so prevalent in the lowlands; but they are generally subject to typhus, dropsy, goitre, and such complaints as indicate constitutional debility. Nor do we find among them either the muscular strength or longevity of the Indians or Africans; and still less of the nations of northern Europe. Are the diurnal changes of temperature to which they are exposed less favourable to health than the alternation of European seasons which expose the frame to changes equally great but less rapid? Or must we rather look for the cause in their domestic habits, which exhibit a strange mixture of effeminacy and discomfort?

When we examine the social or political effects of climate and localities, we are struck with their powerful effects on the past struggles and present state of the country. The cities of the coast must be considered as the inlets both of European products and European ideas. Liberal opinions have extended themselves towards the interior in proportion to local obstacles, i. e. to the greater or less facility of communication. It is this circumstance which marks the difference betwixt Venezuela and the south and centre of Colombia, indicating a distinct and more rapid career of civilization and prosperity. The branch of the Andes which traverses Venezuela is much inferior in elevation to the ridges of Quito and New

* It is the people of colour, or mixture of Africans with Whites and Indians, who on the plains form the most hardy and warlike part of the population of Colombia.

Grenada. The whole of the inhabited part of it belongs to the hot country or temperate mountain zone. The following are the heights of the principal towns through its whole extent:

Caracas	2903 ft.	Mean temp.	71°
Valencia	1495	———	78
Barquisimeto ...	485	———	78
Tocuyo	2058	———	75
Truxillo	2684	———	75
Merida	5280	———	66
Cucuta about	400	———	83

The differences of climate and productions betwixt the different parts of the country are consequently trifling, and form no bar to general communication betwixt the coast and interior. There is therefore an amalgamation of ideas, an homogeneity, if we may use the term, in the mass of feelings and opinions on political subjects. The population is not only more enlightened, but, what is of more importance, more equally so. A different state of things presents itself when we examine the centre and south. The main ridge of the Andes ascends rapidly from the frontier of Venezuela, and, by its direction from north to south, places the population at a continually increasing distance from the sea-ports of the Atlantic; while its superior elevation producing a different climate and temperature, gives birth to new habits and a distinct nationality. To descend to the coast from these altitudes is a matter both of risk and difficulty. The line betwixt the *Llaneros* and *Serranos* is strongly drawn, and a separation of character evident. The country from Cucuta to Bogotà through Pamplona and Tunja has a mean elevation of from 8000 to 10,000 feet, and a temperature of about 59° Fahr. It is true that Bogotà communicates with Europe by the valley of the Magdalena; but the length and inconvenience of this channel of intercourse render it accessible but to few. Hence the struggle of opinions in New Grenada, where the civilization of the superior class is out of proportion to that of the bulk of the people.

The Quitenian Andes afford us another powerful illustration of this view of the subject. The following is the line of elevations betwixt Quito and Chimborazo.

Quito	9537 ft.	59° Fahr.
Llactacunga ...	10,285	57°
Hambato		61°
Riobamba......	9377	57°
Guaranda......	9075	58°

The roads which descend to the coast of the Pacific are

few, almost impassable, and lead to no seaport of importance except Guayaquil. Journeys thither are undertaken with fear and hesitation; and the character of the *Serranos* is marked with all the traits of isolation resulting from the geography of the country.

Next to the direct influence exercised by climate on the frame of man, we may consider it relatively to the facility it affords of nourishing him, and advancing his progress in civilization. The most important presents made by the Old to the New World are cattle and cerealia. The only domesticated quadruped known to the Indians was the llama, which furnished, like the sheep, with thick wool, unwillingly descends or is propagated in the sultry lowlands. The horned cattle of Europe, on the contrary, have multiplied almost equally on the plains as on the *paramos*. On the farm of Antisana, for instance, at an elevation of from 12,000 to 16,000 feet, there are no less than 4000 head. The herds raised on the plains of Venezuela, as on the *Pampas* of Buenos Ayres, are, or were, previous to the revolution, almost countless. Two immense magazines of animal food are thus placed at the two extremes of temperature, in situations uninterfered with by agricultural labour. The horse has been destined to figure in the political changes of the New World. The fear and respect with which he inspired the natives at the period of the Conquest is well known. Horses have since multiplied prodigiously in all parts of the country, but more especially in the plains of Venezuela. There, during the war of independence, Paez, and other guerilla chiefs, at the head of an irregular cavalry, and maintained by the cattle, defied the efforts of the Spanish infantry, and kept alive the embers of the revolution.

The best kinds of horses are those that are bred in the lowlands, and brought to the mountains at about four years old, where they acquire hardihood by the influence of a colder climate, and their hoofs, accustomed only to soft pastures, are hardened on a stony soil.

The breed of sheep, like that of llamas, is limited to the loftier regions of the Cordillera; while goats multiply more readily on such parts of the low country as are both hot and barren, as in the province of Coro, where they form the chief wealth of the inhabitants.

But while nature facilitates the dispersion over the globe of certain species of animals, she seems to limit others by an impassable barrier. The dog undergoes the fate of his European master; his sagacity and strength decay in a hot climate, and the breed dwindles rapidly into an animal totally inferior in habits and organization. The foresters accord-

ingly, and the Indians of the lowlands, who are accustomed to the chace of the wild hog, bring dogs for the purpose from the mountains, where, though the Spaniards are by no means curious in this particular, a strong species of greyhound, more or less degenerated, is to be met with, and is used in the highlands for stag-hunting.

The influence of temperature, and consequently of local elevation, on vegetable life, was first examined in Colombia by a native of Bogotà, the unfortunate and illustrious D. Josè Caldas, who fell a victim to the barbarity of Murillo in 1811, in consequence of which his numerous researches in natural history were almost entirely lost, with the exception of some papers published in the *Seminario de Bogotà* in 1808, and fragments still existing in MS. or casually preserved and printed in Europe, to one of which I shall presently have occasion to refer. Humboldt travelled through South America about the same time that Caldas was directing the attention of his countrymen to physical science, and his investigations have fortunately been subjected to a less rigorous destiny. His admirable treatise "*De distributione Plantarum geographica*," has left for future observers little but to corroborate the accuracy of his views, and multiply facts in illustration of his theories.

When we begin our observations from the level of the sea we find certain families of plants which scarcely ever rise to above 300 or 400 feet; the "*Sandalo*" producing the balsam of Tolu, the *Lecythis*, the *Coccoloba*, the *Bombax*, the *Rhizophora Mangle*, the Manchineel. A second and more numerous class push on to about 2000 feet of elevation; such are the *Plinia*, the "*Copàl*," the "*Anime*," the "Dragon's blood," the mahogany tree, the "*Guayacàn*." Among plants, the *Cæsalpinia*, *Ipomœa quamoclet*, most of the *Bignonias*, *Portlandias*, the Vanilla, *Cassia alata* and *riparia*, the *Pontaderia*, which forms the ornament of tropical rivers. The palms ascend to the height of 5000 feet. The arborescent ferns, from the level of the sea amid the damp forests of Esmeraldas to 7000 feet. Of cultivated plants the Cacao and indigo are most limited as to elevation, neither of which are cultivated with success at above 2000 feet. An attempt to raise indigo at Mindo (3960 feet) completely failed. It would seem that a dry climate is most favourable to indigo, such as is found in the valleys of Aragua near Valencia; while heat and moisture, as Humboldt observes, are particularly required for cacao. Yet cacao cultivated on lands which are flooded part of the year, as is the case with the greater part raised in Guayaquil, is of inferior quality, scarcely pro-

ducing in the market a dollar per cwt. That of Esmeraldas, on the contrary, where notwithstanding the moisture of the climate the waters never settle on the soil, is of equal or superior quality to that of the valley of Tuy near Caracas. In Canigue, at an elevation of about 1000 feet, the trees are loaded with fruit in less than two years from the time of sowing the seed; while generally three years is the period at which they are reckoned to commence bearing.

Coffee is abundantly raised from the level of the sea to elevations of 5000 or 6000 feet, or even higher in favourable situations. There are plantations near the valley of Baños in Quito at above 7000 feet.

Cotton requires, according to Humboldt, a mean temperature of not less than 64°—60°, which would bring it to the elevation of Loxa.

The sugar cane is cultivated in Colombia from the level of the sea to an elevation, which may appear extraordinary, of 7865 feet in the valley of Baños at the foot of Tunguragua, of 8500 in the valley of Chillo below Quito, and of nearly 9000 feet near the town of Hambato. It must be observed, however, with respect to the latter, that the *vegas* or nooks formed by the windings of the river, where alone it is raised, are so sheltered as to produce an almost artificial temperature. A palm tree brought young from Guayaquil flourishes there, and "*Aguacates*," (the fruit of the *Laurus persea*) ripen perfectly, with oranges, limes, and other fruits which in general are not cultivated at above 6000 feet. In proportion, however, to the elevation is the time required for ripening the sugar-cane, varying from nine months at the elevation of 1000 feet, to three years at the elevation above cited.

Plantains and maize are the principal articles of food in the lowlands or hot country, "*tierra caliente*," to use the expression of the natives. The larger variety of plantain, "*Platano harton*," cannot be cultivated at elevations above 3000 feet, while the smaller variety "*Camburi*," will ascend to 6000 feet. Maize is perhaps the plant which, of all others, embraces the greatest variety of temperature and elevation. It is cultivated with equal advantage from the level of the ocean to the flanks of the Andes, 0 to 11,000 feet; temperature 80°—59°. It is true, that in the lowlands it ripens in three months, whereas on the table lands of the Andes it requires ten; but the grain is larger, and the ear fuller in the cold than in the hot country.

The central or temperate zone of the Andes is distinguished by the *Cinchonas*, the arborescent ferns which precede and accompany the palms nearly, and in the moist forests of the

Pacific, entirely to the level of the sea*. At the back of Pichincha they first appear about 8500 feet. The *Alstrœmerias* and *Calceolarias,* peculiar to the New World, belong to this zone, though the former ascend to 11,000 feet and the latter to 15,000.

The *Cerealia,* with almost all the varieties of European vegetables, belong to this region. Humboldt observes a peculiarity that wheat is grown near Vittoria at the elevation of 1700 feet, and in Cuba near the level of the sea; (*Geo. Pl.* p. 161) but it is probable that the reason why the cerealia are cultivated only at elevations where the *Musæ* disappear, may be the natural inclination of the inhabitants of the warm country to prefer the cultivation of a plant which yields an equal abundance of food with infinitely less labour, not only in the mere cultivation, but in the subsequent preparation. The three great wheat districts in Colombia are the mountain chain of Merida, the elevation of which rarely reaches 5000 feet, with a general temperature of 72°; the plain of Pamplona, Tunja, and Bogotà, elevation 8000 to 10,000 feet; temperature 58°; and the Quitenian Andes of the same height and temperature. Humboldt has accurately observed, (*Geo. Pl.*, p. 152) that a comparison betwixt annual mean temperatures of Europe and the elevated tropical regions would by no means give a correct state of the climate. Thus, though the mean temperature of the South of France and of Quito be the same (about 59°), such fruits as peaches, apricots, pears, figs, and grapes, which ripen in perfection in the former, although abundantly produced in the latter, never attain their proper size or flavour. The reason is that the temperature is equal throughout the year. There is consequently no period, as in Europe, of summer heat sufficient to ripen fruit requiring at this season a mean temperature of 65° or 70°. As far, however, as the height of 7000 feet all kinds of fruit are cultivated with success; and the markets of the colder country are thus constantly supplied from the neighbouring valleys or "*calientes*." Humboldt is mistaken in supposing the olive always barren (*semper sterilis manet*, p. 154.). On the Quitenian Andes near Hambato it produces abundantly, though little attention is paid to its cultivation.

When we ascend above the extreme limit of cultivation, which may be placed at 11,500 feet, and pass the region of the *Barnadesia, Hyperica, Thibaudiæ, Gaultheriæ, Buddleiæ,* and other coriaceous leaved shrubs which, at this elevation, form thickets of perpetual bloom and verdure, we

* Humboldt, who had not visited these forests, confines them to betwixt 800 and 260 hexap. *De Geo. Pl.*, p. 185.

enter the region of *Paramos* (13,000 to 15,000 feet) properly so called, which present to the eye unvaried deserts clothed with long grass, constituting the pasture grounds of the Andes. Humboldt is inclined to fix below this region the limit of forest trees; (*Geo. Pl.*, p. 148) and in fact very few are generally met with near this elevation on those flanks of the Cordillera which join the inhabited table lands. But I have observed on crossing the side of Pichincha, towards the uninhabited forests of Esmeraldas, that the forests occur nearly through the whole space which, on the eastern slope, is a naked *paramo.* Is this owing to a difference of climate? Or has the practice of burning the *paramos,* universal in the Andes, together with the demand for fire-wood in the vicinity of large towns, contributed to give this region the bare aspect it has at present? Further observations on the mountain slopes towards Maynas and Macas are necessary to throw light on this point. It is certain from the present aspect of the inhabited plain of Quito, where we meet with a few scattered trees of *Arayan* (*Myrtus*) and artificial plantations of *Capuli,*(*Prunus salicifolia*) we should conclude that the region of forests had scarcely ascended to the height of 8000 feet, yet some of the houses of Quito are still standing, built with timber cut on the spot.

A circumstance which cannot have escaped the notice of those who have ascended towards the limit of perpetual snow, is the variety and luxuriance of the Flora at the very point where the powers of vegetation are on the brink of total suspension. At above 15,000 feet the ground is covered with *Gentianas,* purple, azure and scarlet; the *Drabas,* the *Alchemillas*; the *Culatium rufescens* with its woolly hood; the rich *Ranunculus Gusmanni*; the *Lupinus nanus* with its cones of blue flowers enveloped in white down; the *Sida Pichinchensis* spotting the ground with purple; the *Chuqueraga insignis*; all limited within a zone of about 500 feet, from whence they seem scarcely to be separated by any effort at artificial cultivation. Several attempts I have made to raise the Gentians, *Sida,* and other plants of the summits of the Andes, at the height of Quito, have been invariably unsuccessful. The attempts indeed to domesticate plants in a situation less elevated is attended with greater difficulties than the transport of plants from one climate to another. Besides the difference of atmospheric pressure, as Humboldt has observed, plants transferred from one elevation to another never meet, for a single day, with the mean temperature to which they have been accustomed; whereas transferred from one latitude to another, the difference is rather in its duration than in its in-

tensity. It is easier to accustom a plant of the lowlands to this elevation than to bring down those of the *paramos*. Thus the orange and lemon trees, Aguacates (*Laurus persea*) *Ricinus communis*, *Datura arborea*, all natives of the hot lowlands, grow and flourish, more or less, at an elevation of 8000 feet above the level of the sea.

[To be continued.]

XX. *Observations on Malaria, with Suggestions for ascertaining its Nature: read before the Literary and Philosophical Society of Manchester, November* 15, 1838. *By* Thomas Hopkins, *Esq.**

MALARIA is considered the scourge of a considerable portion of Italy, where it is spoken of with an undefinable feeling of horror. It is not, as its name may seem to imply, supposed to be simply bad air, but a poisonous effluvium arising from some undiscovered operation of nature, and known only through its dreadful effects on human beings. The volcanic nature of the country, the gases thrown from its surface by the agency of internal heat, with their offensive smells, are in the imagination of the people connected with the cause of marsh fever, and designated by the word Malaria. The author of the description of Latium speaks of the volcanic soil being prejudicial to the atmosphere; and Mrs. Starke, in her account of Rome, talks of the sulphur, arsenic and vitriol which abound, producing malaria. This lady may be considered as speaking the ordinary language of the people of the country on the subject. Learned writers too have treated of malaria in a way almost equally mysterious, among whom may be named Dr. Macculloch. The Doctor does not indeed suppose that sulphur or vitriol is instrumental in producing the poison, but that it is the product of vegetable fermentation or putrefaction, the form and mode of operation of which we are not able to trace, becoming conscious of its existence only by witnessing its effects. The pest is shown to be worse in Italy than in other parts of Europe, but the Doctor has shown that it really prevails in other countries, and to a greater extent than had been previously suspected. Whatever may be the peculiar nature of malaria, Dr. Macculloch seems satisfied that it does not arise from the presence of minerals, nor from local causes confined

* Communicated by the Author. The reader may advantageously compare this paper with Mr. Addison's "*Remarks on the Influence of Terrestrial Radiation in determining the site of Malaria,*" Phil. Mag. and Annals, N.S., vol. iv. p. 272, *et seq.*—Edit.

to any particular country. It is indeed distributed extensively over the surface of the earth. Asia, Africa, and America feel the scourge in a more violent degree than it is felt in any part of Europe: the plains of Bengal, the vallies of the African coast, and the West Indian islands are infected in a more deadly degree than even Italy; it is therefore reasonable to infer that the locality of gases or minerals is not connected with its production. It is found too in climates having different temperatures to those of the countries just named, as on the eastern coast of England, in France, and Holland: very high temperature therefore does not seem essential to its production. But though not absolutely requisite to its production, high temperature appears to increase its virulence, the injurious effects on the human constitution having a palpable relation to the temperature of the atmosphere. Thus in Lincolnshire, France, and Holland, its operation is slow, and it requires considerable time to produce fatal effects; but in the Campagna of Rome, it is said that a single night passed within the full influence of the pest endangers life. In the jungles of Bengal it is fully as bad as in the Campagna, and in the vallies of the western coast of Africa the greater number of the crew of a ship have been known to die in a short time. Thus it becomes apparent that the virulence of the fever arising from malaria is great in proportion to the heat of the climate.

Yet great heat alone does not seem capable of producing the poison. However high the temperature may be, provided it is not accompanied by moisture, the air is found to be healthy. The plains of Russia are hotter in summer than the marshes of Holland, but no malaria is found in the former, while it abounds in the latter, because the plains of Russia are dry as well as hot. Rome is only seven degrees hotter than Moscow in the hottest month of the year, yet malaria is virulent in the one, while it does not exist in the other. In the sandy deserts of Asia and Africa we have perhaps the hottest climates of the globe, but as these deserts are at the same time dry malaria is not prevalent in them. The inference is that heat alone will not produce malaria, but that the presence of moisture also is necessary.

Dr. Macculloch admits, "that the extreme of evil from malaria occurs in tropical climates, it appearing almost proportioned to the heat of the climate, and, what is important to observe, to the moisture also." He also remarks that "Egypt is free from the fever arising from malaria except at the period of the subsidence of the Nile, unless where, as at Damietta, the cultivation of rice is pursued." But the Doctor

afterwards attempts to show that heat and moisture are merely agents in the production and distribution of a poisonous exhalation from vegetable matter, which exhalation he says is the true malaria. A moist air, says he, "is the best conductor of the malaria, as moisture in the air, under the form of evening mists or in other modes, appears to be even its proper vehicle or residence." And again he says, "water in some form is necessary to the production of that peculiar vegetable decomposition which is the source of this poison; and the action of moisture is twofold, inasmuch as it not only accelerates vegetable decomposition, but renders the atmosphere a fitter conductor of the poison." It is not here pretended that this poison which is stated to be the result of vegetable decomposition has ever been detected in a palpable form. It is not asserted that its existence as a peculiar substance has been ascertained. It is only from effects produced on human health that its existence is inferred. Moisture, says the Doctor, is not capable of poisoning the atmosphere; and as he finds that the atmosphere is poisoned, he infers that the poison is exhaled in the decomposition of vegetable substances.

There is a prevalent belief in many parts of the world that fogs coming from the sea produce fevers of the same kind as the malaria fever: this belief is evidently at variance with the theory espoused by Dr. Macculloch, and he thus reasons with those who entertain it. "The proof that it is malaria in the fog, and not the fog itself which is the cause of disease, is evinced by the following facts: No intermittents are ever produced on the western or northern shores of the island, and for the plain reason that there is no land whence they may arrive. The clouds of mountainous regions do not produce fevers though these are also fogs; and what forms a most absolute proof of this is, that in Flanders it is the fogs which come with a south-west wind or the southerly winds which transport and propagate malaria and diseases, while as soon as the winds shift and blow from the sea the fevers disappear, though these particular winds are so charged with fog as to darken the whole country for days." These are the facts on which the Doctor relies, and he remarks, that "it ought surely to be unnecessary to say that if fogs alone could produce such fever water itself must be the poison, since a fog is a cloud, and its constituents when pure are only atmospheric air and water."

With respect to the first alleged fact, that no intermittents are produced on the western or northern shores of our island, the reason for it may be found in the circumstances of these

shores being either rocky, or well drained and under cultivation, and consequently dry. They are also nearer to the Atlantic Ocean, and therefore cooler in the summer than the eastern coast. These two causes may be sufficient to account for the difference which is found on the eastern and western coasts of this country. But I have been informed by Dr. Briggs of Ambleside, formerly resident in Liverpool, that in his younger days autumnal agues were common on the low grounds of Lancashire, particularly in that part called the Fylde country, and that they occasionally prevail at present. Their diminution may be attributed to the better drainage of the country, which has converted it from a comparative marsh to dry tillage land. That the clouds of mountainous regions do not produce fevers may arise from their low temperature. And as to the fact that south-west and south winds produce fevers in Flanders, while on a sea wind coming which covers the country with fogs the fevers disappear, this may only prove that the sea wind being a cold one, the reduction of the temperature caused the fever to cease. The Doctor seems to think that there is great force in the remark "that if a fog alone could produce fever water itself must be the poison:" but the argument may not, as we shall presently see, turn on the water in the atmosphere as water, but as steam or elastic vapour. And it may be found that when there is a certain quantity of steam in the atmosphere at a high temperature, disease may be a consequence, notwithstanding that water is wholesome in a liquid state. Dr. Macculloch's opinions respecting the cause of malaria are at variance with numerous and well ascertained facts. This pest is found on sea borders and islands, as on the coasts of Italy and Africa, on the small Maldive islands in the Indian Ocean, in the West Indian islands, even in Barbadoes, which pushes out east a considerable way into the Atlantic; it is also found at sea at great distances from land, and beyond the reach of effluvia from vegetable putrefaction. In the valuable statistical report of sickness and mortality among the troops in the West Indies, recently laid before the House of Commons, it is said, when speaking of the hypothesis of vegetable exhalation producing malaria,—"Were this hypothesis correct, we might expect that British Guiana would, from its proximity to this cause of disease, be most subject to its operation, and consequently the most unhealthy, and that the colonies further to the north, being least exposed to it, would enjoy the greatest degree of salubrity. The result of our investigations into the comparative mortality in each colony shows however that their relative salubrity is by no means

affected by their proximity to, or distance from, the continent." (See table of relative mortality at the end.)

The atmosphere in which we live, it is well known, has within it azotic gas, oxygen, a small portion of carbonic acid gas, and invisible vapour of water, or steam. The nature of the three first-named substances, as far as respects their influence on animal life and health, is tolerably well known; but the same cannot, I believe, be said of the steam which exists in the atmosphere; nor am I aware that any scientific attempt has been made to trace its influence on the animal œconomy with relation to health and disease. It is known that too dry an atmosphere is productive of unpleasant and sometimes of very painful consequences. Evaporation from the body goes on so freely as to deprive it in too great a degree of moisture and to produce a constant thirst. A certain degree of moisture in the air keeps the body soft and gives that clearness of complexion common in many parts of the north-west of Europe, which is supposed to indicate vigorous health. But in other parts of the world there is, with reference to the animal œconomy, a superabundance of steam in the atmosphere. This is popularly recognised in those parts when it takes the form of dampness, as it is then thought to be injurious to health; but it has not been treated of scientifically. In the absence of any treatise on the subject I propose to call attention to a few points, in the hope that it may induce others to institute a full inquiry, meteorological, chemical and physiological, such as the important nature of the subject demands.

Let us imagine that at some certain time no vapour existed in the atmosphere: according to the known laws of evaporation vapour would immediately begin to arise from all wet surfaces; and supposing evaporation to proceed, the whole of the space occupied by the atmosphere would soon be filled with vapour, or invisible steam; the quantity being proportioned to the temperature of each particular place. Where the temperature was low there would be but little steam, where it was high there would be more, and every part would in time have its maximum quantity, when of course no more could rise. In this state, as there would be no condensation of vapour, there would be no clouds and no rain. But owing to the unequal influence of the sun on different parts of the earth's surface, and the diurnal motion of the globe, this state of things does not exist. Between the tropics the sun vaporises water freely, and at the same time by its heat rarefies the air. The vapour ascends with the rarefied air to the upper regions of the atmosphere, cold air flows in below

from parts nearer to the poles, and the process is repeated and continued. The vapour that is taken up with the rarefied air flows with it through the higher region towards the poles, and becoming cooled in its passage, a portion of the vapour is deposited in the form of rain. The air thus cooled and deprived of part of its vapour returns from each polar region, flowing along the surface of the earth, and has its temperature increased by the sun's heat as it advances towards the equator; it consequently becomes a dry air, or is disposed to take up vapour from moist surfaces. Thus we see that in accordance with the general laws of nature, the air near to the surface of the earth which is flowing from the poles to the equator is a dry air.

But through the influence of local causes this course of nature, though general, is not universal. While in some parts the air near the surface of the earth is very dry, in other parts it is so fully charged with moisture as to prevent further evaporation taking place at the existing temperature. In this case, the dew-point, being that degree of the thermometer at which dew is formed from the atmosphere, is the same as the temperature. In different parts of the world there are various degrees of dryness in the atmosphere, and these are expressed by the relation which the dew-point bears to the temperature. When the dew-point is only one degree below the temperature evaporation goes on very feebly; when more than one, it proceeds more vigorously; when at ten, fifteen, or twenty degrees below the temperature, evaporation goes on with proportionally increased energy. Dr. Dalton made various observations on the subject near to and on the mountain of Helvellyn in Cumberland. He found that at one time, in the valley below the mountain, the temperature was 70°, the dew-point 53°, difference 17°; at another time the temperature was 56°, the dew-point 46°, difference 10°: of course the energy of evaporation would in each case be proportioned to the difference. At the same time, on the mountain, 855 yards above the valley, the temperature was the first time 56°, dew-point 46°, difference 10°. The second time it was 46°, dew-point 42°, difference 4°.

Thus in these four observations the energy of evaporation would be as the numbers of the difference, or as 17, 10, 9, and 4. In the same paper in which the above facts are to be found, published in the 4th volume of the Society's Transactions (Manchester) the Doctor has given a table of numbers exhibiting the drying power of the atmosphere, which is one form of expressing the energy of evaporation.

Dr. Dalton also states that in twenty years of observation

chiefly at Manchester, he found that in the months of June, July, and August the dew-point generally ranged from 50° to 60°; that it was only once at 64°, once at 63°, five times at 62°, three times at 61°, and twenty times at 60°. But in other parts of the world where malaria prevails, different meteorological facts present themselves, and furnish ground for presuming that the laws of evaporation affect the people of those countries differently to what they do the inhabitants of our colder climate. In Rome, in the hottest month of the year, during the day the temperature ranges from 90° to 100° in the shade, and the air is damp; the dew-point must therefore be high.

In Captain Alexander's observations on the western coast of Africa, we are told that "four days after leaving Teneriffe, while proceeding for the river Gambia, on the 6th of October, the wind at south-east swept over the ocean charged with moisture. At noon the thermometer under the awning was at 80°, while with Daniell's hygrometer I found the dew-point at 70°. On the 7th of October during a sirocco the thermometer was at 86°, the hygrometer 76°. At the end of November in the Bight of Benin, in sight of the island of St. Thomas, the temperature was 84°, the hygrometer 79°!" In the meteorological register kept by Mr. Oldfield, surgeon in Laird and Oldfield's expedition up the Niger, we find it stated that during the month of April, on the river, the temperature was generally above 100°, and on the 14th of April it reached 118°. No hygrometrical return is given, but as the air is stated to have been damp, it may safely be inferred that the dew-point was extremely high.

Here then we have instances where the dew-point was in different places at the respective heights of 42°, 46°, 53°, 60°, 70°, 76°, and 79°, and from Oldfield's register it may be presumed to have been much higher on the Niger, probably 90°. Suppose water of the temperature of 98° to be placed in these various atmospheres, and it will be seen that the energy of evaporation of this water would be very different in those different places. Evaporation would go on much more freely when the dew-point was at 42° than when at 60°, at 60° than when it was at 70°, at 70° than 76° or 79°; and the nearer the dew-point approached to 98°, the temperature of the water, the more feeble would be the evaporation. Now the temperature of the human body in its healthy state being 98°, when this body is placed in an atmosphere the dew-point of which is 42° or even 60°, evaporation will go on vigorously. Let the dew-point rise to 70°, and still evaporation might possibly go on with considerable energy; but should the dew-point

be raised to 80° or 90°, evaporation from the body must become very feeble.

In a paper read March 1830, and printed in the 5th vol. of the Society's Transactions, Dr. Dalton has shown that a healthy man taking daily into his stomach 53 ounces of fluid gives off from the external skin in the same time $6\frac{3}{4}$ ounces of water, and from the lungs $20\frac{1}{2}$ ounces, making together $27\frac{1}{4}$ ounces. Now is it not clear that this important process in the animal œconomy must be variously affected by the hygrometrical state of the atmosphere; differently when the dew-point is at 50° to what it will be when at 60°, 70°, 80°, or 90°? Were the dew-point to be carried up to 98°, this process, it would appear, must stop, evaporation would cease, and the $27\frac{1}{4}$ ounces of water would remain in the system, or be disposed of by nature in some different mode, which would constitute a material derangement of the animal œconomy. Again, it requires a considerable portion of caloric to vaporise $27\frac{1}{4}$ ounces of water; and when this quantity of water is daily thrown off by evaporation, it is presumed that the requisite quantity of caloric is abstracted from the human body. Evaporation seems indeed to be the agent that nature employs to regulate the temperature of the body, and when it is stopped or materially impeded fever generally ensues, the temperature of the body rises, and from 98° goes up at last to fever heat or 112°. Lavoisier and Seguin estimated the average loss by perspiration from the skin and lungs in twenty-four hours at 2 pounds 13 ounces, of which 1 pound 14 ounces were dissipated by the skin, and 15 ounces by the lungs. (*Traité Elémentaire de Chimie*, 3me *édition*, 228.) The degree of dryness of the atmosphere may, it is obvious, influence the whole quantity evaporated, and also the proportions given off respectively by the external skin and lungs.

Malaria seems not to prevail in a virulent state where the dew-point is below 60°. In Lincolnshire and parts of Holland and of France the dew-point is probably sometimes above that height towards the end of the summer: in those countries malaria fever prevails in its mildest form, but always at those periods when the dew-point is presumed to be the highest. In the maremma of Tuscany, the Campagna of Rome, and other parts of the south-west coast of Italy, in the latter part of the summer the dew-point must be high, and precisely at this period of the year in these places malaria prevails, and in the worst form in the hottest and dampest parts. In the West Indies, in Bengal, and in the African valleys the same facts are observable; malaria is always viru-

lent in proportion to the height of the dew-point. It is however greatly to be lamented that we have not more particular and full hygrometrical returns of the state of the atmosphere in those parts of the world; were we furnished with such returns, there is little reason to doubt that it would be easy to show, by the evidence of facts, that there is such a general coincidence of a high dew-point and the prevalence of malaria fever as would place them in the relation of cause and effect. In the returns from the West Indies, published in the Statistical Report of the mortality there, we have a monthly hygrometrical return for the year 1832, from the island of St. Vincent: it is the only one in the whole report, and the substance of it is given at the end. It will be seen from this that the mean of the dew-point for the year is 68°·86; the lowest, in the month of February being 67°·14, and the highest, in July, 70°·25.

From the laws of evaporation as ascertained by experience, it is known that the rest or motion of the air has considerable influence on evaporation from wet surfaces. When vapour rises from these surfaces it remains for some time resting upon and near to them, where it checks further evaporation. But when a current of air by its mechanical action carries away the newly formed vapour, fresh vapour immediately escapes, and the process is repeated: the influence of winds in drying is a familiar instance of this fact. The human body is disposed to give out vapour to a given extent to an atmosphere, the dew-point of which is, say 70°; but without motion in the air the vapour first given out would impede further evaporation; a wind removes this impediment and suffers the evaporation to go on more freely. In accordance with this it is known that malaria fevers more commonly prevail in a warm damp atmosphere when it is stagnant! A brisk wind is said popularly to purify the air, and to take away mephitic vapours; may it not merely facilitate evaporation? Persons suffering under malaria fevers are relieved by fanning! Were the air charged with poisonous vegetable miasmata, it would be reasonable to conclude, that fanning, by bringing more of this poisoned air into contact with the patient, would increase the disorder! But if the view here taken be correct, the cause of the relief that is felt becomes sufficiently obvious; the evaporation which was checked by the previous accumulation of vapour is accelerated by its removal.

In temperate climates the heat of the atmosphere is considerably below that of the human body, and the dew-point is ordinarily below the temperature of the atmosphere. But with reference to perspiration it seems to be as important that

the dew-point should be below the temperature of the atmosphere, as that the temperature of the atmosphere should be below that of the human body, unless the temperature be very low. The temperature and dew-point being the same, and at 60°, evaporation would not proceed rapidly from the human body in a stagnant atmosphere. But if a much colder atmosphere, say 40°, were to be fully saturated, the body being 98°, would warm the air immediately around itself, give it a capacity to take up more steam, and at the same time create a more decided upward current that would continually change that part of the air which was in immediate contact with the body.

Writers and travellers when speaking of a damp atmosphere commonly restrict their remarks to those atmospheres which discharge rain, or are charged with fogs, their object being generally to intimate that such atmospheres wet or moisten substances exposed to them. But an atmosphere may be saturated with steam, without giving out any part of it to substances immersed in it which are of an equally high temperature. Suppose both the temperature and the dew-point to be 70° during the night, neither rain would fall nor dew be formed, and yet with reference to evaporation it would be a damp atmosphere, seeing that no evaporation could take place in it from water of the same temperature. It is the existence of transparent elastic steam in certain quantities in the atmosphere which prevents further evaporation from wet surfaces, and it is this steam we have now under consideration, and not either the fall of rain or the floating of condensed vapour. These are effects of this elastic steam having previously existed; but it is the steam itself of which we are now treating, and of its effects in checking evaporation. An atmosphere of the temperature of 118°, as found by Oldfield in Africa, might possibly have a dew-point of 98°, and would consequently have a drying power of 20 degrees, but the transparent elastic steam in this atmosphere would put an entire stop to evaporation from the human body, seeing that the temperature of that body in a healthy state does not rise higher than 98°. It is therefore necessary to observe that it is neither the rain that falls, nor the condensed vapour that floats in damp climates where malaria prevails, that is here supposed to constitute that malaria, but solely the quantity of invisible steam which, by its mechanical pressure on the surface of the skin and lungs, prevents the ordinary process of healthful evaporation from being continued, and thus this invisible steam becomes the real malaria. When this steam stops evaporation from the body, the $27\frac{1}{4}$ ounces of water previously

thrown off by evaporation remains in the body, with the heat requisite to evaporate it, and this may cause fever, and carry the temperature of the body up to 112°.

The evidence of what is called dampness of the atmosphere, as given by writers generally, is of a loose and unsatisfactory nature. Even in the valuable report from the West Indies already alluded to, the intelligent compiler says: "If the mortality of the troops depended materially on the influence of moisture, we might expect it to attain its maximum in those stations where the greatest fall of rain takes place, whereas the average mortality of troops in Jamaica is at least double that which prevails among those in British Guiana, though the quantity of rain which falls in that island is little more than one half as great as in Guiana."

Here we see that the quantity of rain that falls is taken as evidence of the moisture of the climate, whereas it is quite possible that rain may fall from a considerable elevation while the dew-point is comparatively low near the surface of the earth, while on the other hand the dew-point may be very high without much rain falling. When the autumnal rains fall at Rome malaria is diminished, but those rains by cooling the country lower the dew-point or reduce the quantity of steam in the air. In the summer in Great Britain the dew-point is at times 15 or 20 degrees below the temperature when clouds are forming and rain falling from a considerable elevation. The fall of rain is evidently not an indicator of the state of the dew-point near the surface of the earth.

In directing our attention to this subject it may be worth while to observe, that it is not so much the mean temperature of certain places that should be noticed, as the high temperature of the days vaporising much water, and thus raising the dew-point very high. A temperature of 100° in the day and 60° at night, making a mean of 80°, might in a marshy country, such as that near Rome, give a dew-point of, say 90°, at or near sunset, while a uniform temperature of the mean 80° could not possibly give a dew-point of 90°, nor above 80°. A day temperature of 70° and a night temperature of 40°, making a mean of 55°, might give a dew-point of 60°, but a uniform temperature of the mean could not give so high a dew-point as 60°. As the former may be taken to represent the Campagna of Rome, the latter may be considered to represent the marshes of Lincolnshire. Now when on the going down of the sun the temperature of the Campagna sunk to 90°, the temperature and the dew-point might be the same; and when in Lincolnshire the temperature sunk from 70° to 60°, the temperature and dew-point might also be

the same. But the dew-point the same as the temperature even at 60°, with a stagnant atmosphere, might seriously check evaporation from the human body, though not to the same dangerous extent as the higher dew-point in the Campagna of Rome.

Captain Cook and others experienced the unhealthy influence of hot and damp air at sea, far removed from the supposed seats of poisonous effluvia from decaying vegetable substances, and also found the benefit of heating and drying the air. In Cook's Voyage from 1772 to 1775, p. 9, it is stated that, "in latitude 3° north, August 20 to 27th, the thermometer generally at noon kept from 79° to 82°. On the 27th spake with Captain Furneaux, who informed us that one of his petty officers had died. At this time we had not one sick on board, though we had everything of the kind to fear from the rain we had had, which is a great promoter of sickness in hot climates. To prevent this I took every necessary precaution by airing and drying the ship with fires made between decks, smoking, &c. &c.; neglect of these seldom fails to bring on sickness, but more especially in hot and wet weather." And in page 291, vol. ii. "Care was taken to keep the ship clean and dry between decks. Once or twice a-week she was aired with fires. I had also frequently a fire made in an iron pot at the bottom of the well, which was of great use in purifying the air in the lower part of the ship." Sir J. Pringle in his account of Cook's sanatory precautions, says, that "some old ships were more healthy than the new ones, because the former having their galley in the forepart of the orlop, the chimney vented so ill, that it was sure to fill every part with smoke. This was a nuisance for the time, but, as he thought, abundantly compensated by the extraordinary good health of the crews." Perouse, when proceeding from the northern part of the Pacific to the Equator, and in the latitude of 10° north, writes thus: "The heat was suffocating and the hygrometer had never indicated more humidity since our departure from Europe. We were breathing an air destitute of elasticity! which joined to unwholesome aliments diminished our strength, and would have rendered us almost incapable of exertion if circumstances had required it. I redoubled my care to preserve the health of the crew during this crisis produced by too sudden a passage from cold to heat and moisture. I ordered the ship to be dried and ventilated between decks." A high dew-point, no doubt, was here the cause of the illness, and the drying was merely heating the air so as to carry the temperature much above the dew-point. On the 20th of January the brothers Lander sailed in the

ship Carnarvon from the island of Fernando Po, having "a crew of seven European seamen, two free negroes, one Krooman, one captain and two mates. Two of the seamen were ill of fever, Owen Williams and C. Hall. On Sunday, January 23, one of the sick seamen died. On January 26, three of the healthy men, namely, the steward, the second mate and a seaman, were taken ill of fever. January 27th, a seaman taken ill of fever. The weather calm, with light winds; the island still in sight! January 30, another seaman taken ill of fever. The steward died. February 4, the captain taken ill. John Williams died. February 6, the chief mate taken ill of fever. February 7, Smith, seaman, died." Here there was doubtless a high dew-point, and the temperature was not, as in the instances of Cook and Perouse, raised much above it by fires. It would be very easy to give ample additional evidence of the pernicious effects of damp air at sea far beyond the influence of vegetable effluvia.

But it may be thought that, if warm and damp air is sufficient to produce fevers, the sea between the tropics would be found more unhealthy than the land, which is known to be contrary to experience. To this it may be replied that the ordinary cool current of air that flows from the poles to the equator is in general sufficiently dry even over the sea to prevent that part from being very unhealthy, or at least as much so as the hot and moist valleys between the tropics. Captain Basil Hall in his fragments of voyages says: "As we approached the equator the thermometer fell from 82° in the day to 79° or 80° at night. The symptoms of change of climate became daily more manifest. Every skylight and stern-window was fastened wide open, and every cabin scuttle driven out that a free draught of air might sweep through the ship. The seamen and marines dined on the main deck that the lower deck might be kept as cool and as airy as possible against the sultry and feverish night season. We generally exposed a dozen buckets full of sea-water on the gangway at 8 or 9 o'clock in the evening, and these being allowed to stand till the morning, 4 or 5 o'clock, became so much cooler than the sea by the evaporation during the night, that the shock was unspeakably grateful."

Here we perceive that evaporation was going on actively on the surfaces of the water in the buckets, the dew-point must therefore have been considerably below the temperature; yet even here we find the night season described as sultry and feverish, though the temperature was then a few degrees lower than it was during the day: this could arise only from the dew-point being then a little nearer to the tem-

perature. But still the dew-point in Captain Hall's ship must evidently have been much below what it was in the Carnarvon. The above account of Captain Hall furnishes an instance of what has been already stated in a general form, that the heat of the sun rarefies the air, and makes it flow over high in the atmosphere, before it becomes fully saturated with vapour. The most important exceptions to this general law are to be found in those places where malaria most abounds, that is in heated valleys or marshes near to, or within the tropics. The sea at its surface, between the tropics, is seldom found much hotter than 80°; but we have seen that in African valleys the temperature has been from 100° to 118°, and it follows that the latter places are likely to have a much higher dew-point than the former. In some of the African rivers, commanders on losing many of their men and having others ill of fever, have put out to sea in order to get out of reach of the poisonous influence; this however it is clear, from what has been said, may have been only a case where an atmosphere of, say 100°, highly charged with steam, was exchanged for one of 80°, with a dew-point of perhaps only 70° or 65°. The superabundance of steam from heated valleys or flat coasts may however, it is obvious, be taken far out to sea by gentle winds, and thus parts over the sea may be made as unhealthy as hot and damp valleys. This may have been the case with the Carnarvon on board of which were the Landers. But so little attention has been paid to hygrometry by navigators, as to leave us in ignorance of what the dew-points really were.

There seems good reason to believe that a warm and moist air may be rendered more healthful by heating it, because then the temperature would be so much higher than the dew-point as to render it a drying air. But it is still more certain that a reduction of the dew-point much below the temperature removes the cause of malaria fevers: "The Harmattan," says Lander, "a land wind, passes over the sands of Africa, and while it lasts the dryness of the atmosphere produces an unpleasant feeling, but it is said to be not injurious to health." And again, "The effects of the Harmattan after the rainy season are most beneficial in drying up the vapours with which the atmosphere is loaded; and it has been observed, that on the return of this wind at the end of the rainy season, the recovery of invalids commences." But this dry wind seldom continues longer than three or four days. What the state of the dew-point is while it lasts I have never learned, but it must be very low compared with the temperature, as it dries the country with extraordinary rapidity.

That malaria fever is an effect of an excess of steam in the atmosphere, more especially when the temperature is not considerably above the dew-point, may be inferred from various observed circumstances. In the neighbourhood of Rome, as soon as the morning sun has raised the temperature, danger from malaria is much reduced. On the 10th of August the stubble and weeds of the Campagna are begun to be burnt, and it is found that whenever the heat from these fires raises the temperature, the air is for the time partially purified; doubtless because this heat, like that of the sun in the morning, dries the air as well as heats it; that is, raises the temperature to a greater height above the dew-point. It is a common remark in the Campagna, that keeping up a fire in a house during the night purifies the air; and it is well known that in those parts of Rome where the poor people are crowded together there is no malaria, while the thinly inhabited parts are affected. In order to avoid malaria, the wealthy Italians are careful not to sleep on a ground floor. And any one who has observed the way in which a fog rising from a neighbouring marsh or lake creeps along and spreads itself over the lower levels, even when the air appears to be still, will see why rooms of ground floors should be filled with damp air in certain places. The ancient inhabitants of Italy, as may be seen at Pompeii, built their houses round an interior square, the entrance to which could be easily closed; this enabled them effectually to keep out a low stratum of damp air. The streams of fog creeping along the ground as they do, enable us to account also for the local attacks of malaria. A slight current of air confined or turned by a valley, a ridge, a wall, or even a hedge, may take the vapour to a particular part, while other parts, at a small distance, may not be visited by it. The supply being continued from the source, the vapour may be sufficiently dense in certain parts to make those parts unhealthy; but when it expands and by its elastic force diffuses itself, it becomes too thin to saturate the adjoining air up to an unhealthy degree. The same kind of observation will apply to planting woods, or even hedges in particular situations; they may by arresting or turning the sluggish currents of saturated air become barriers. It is said that malaria is never found more than 2500 feet above the level of the sea; this may arise either from the lowness of the dew-point, or the absence of that density of steam which exists only in the lower regions of the atmosphere.

The difference observable between the natives of a warm and damp country, and strangers coming to it from a colder climate, in their respective capabilities of resisting malaria is

striking, but explicable on the supposition that obstruction of the ordinary amount of evaporation is the evil experienced. By the natives, the moisture which cannot find its way out of the body by evaporation, may be thrown off by exudation, while the temperature of the body may be kept down by suitable regimen. But a stranger coming from a cold and dry climate, accustomed to take considerable quantities of heating food into his stomach, and to have much caloric carried off by evaporation, has a sudden stop put to this most important process; the water and fire remain in the system, and fever is the result. Nature deranged in her operations struggles with these new circumstances. The stomach refuses food, because to take it would be heaping new fuel on the body already overcharged with fire. A copious perspiration by exudation sometimes takes place, but mostly when the superabundant fire has been dissipated. Inquiry may possibly show that this is the ordinary expedient of nature under the influence of a heated atmosphere charged with an excess of steam. Perhaps as evaporation is checked exudation increases, and with suitable regimen, the system may in time become adapted to the climate. It is observed that the natives of such climates appear to have a clammy moisture on their skins, and their complexions are sallow. The black race are the least affected by a hot and damp atmosphere; does exudation with them supply the place of evaporation in a greater degree than among the European races?

If the foregoing observations be well founded, it may be presumed that in those parts of the world which have a high temperature malaria will be found, and especially when the air has been some time stagnant, in the following situations, viz.

1. Over the open sea. It will be mild here because the temperature is not very high.
2. Over slowly moving rivers. They will be somewhat more heated by the sun than the sea is, and will therefore evaporate more freely.
3. Over meadows and woods. The great extent of moist surfaces admits of great evaporation from these.
4. Over shallow stagnant water. The temperature of the water will be high, and evaporation consequently great.
5. Over tide sands and muds. These become very hot, and consequently evaporate copiously.
6. Over marshes. These combine great heat, extensive surface for evaporation, and abundant moisture.

With due attention to local influences I would then propose, in order to ascertain whether malaria be or be not the effect

of an excess of steam in the atmosphere, to have registers kept of the thermometrical, hygrometrical, and barometrical states of the air wherever malaria as found to prevail. It would be also more satisfactory if the force and direction of the wind were noted. Such registers may possibly exist at present, but so little attention has been paid to hygrometry, as to make it unlikely that any should be found that would be satisfactory in the detail. The object of the greatest importance is to ascertain the dew-point, that being the point at which evaporation ceases from substances of equal temperature, and from this to any higher temperature is to be deduced the energy of evaporation. Possessed of registers of this description, it would be possible to exhibit scales of the drying powers of the air at all temperatures that are found most conducive to health. Physicians might then be enabled to direct patients to remove to a more moist, or to a dryer atmosphere, as particular cases might require. New modes of prevention and cure might also be devised, such as exposing the patients to strong currents of air, drying the air by heating it, or even by taking the steam out of it by exposure to hot salt, lime, &c. &c. Exudation in a steam or hot air bath might be tried as a succedaneum to evaporation. These are however only speculations, and facts are the things wanted. The object of the writer of this paper has been to invite attention to the subject, in order to obtain the facts requisite to the formation of a more conclusive opinion.

Mean monthly Hygrometrical Return for the year 1832, *in the Island of St. Vincent, as given in the Official Report.*

Jan.	Feb.	Mar.	April.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.
68°·68	67°·14	67°·99	67°·93	69°·30	69°·25	70°·25	69°·66	69°·69	69°·39	69°·41	67°·31

A Table of the Deaths per 1000 *of Strength, and the portion of those who died of Fever, per Annum, of the White Troops in the West Indies, being the average of the returns for the Twenty Years from* 1817 *to* 1836, *arranged in the order of the Mortality. Taken from the Official Report from Twenty-two Stations.*

		Deaths in 1000.	Deaths by Fever.
1.	The Bahamas	200	
2.	Savannah la Mar	200	
3.	Montego Bay	178—9	150—7

		Deaths in 1000.	Deaths by Fever.
4.	Spanish Town..................	162—4	141—1
5.	Tobago	152—8	104—1
6.	Port Antonio	149—3	126—0
7.	Up Park Camp	140—6	120—8
8.	Dominica........................	137—4	49—3
9.	St. Lucia	122—8	63—1
10.	Port Royal	113—1	93—9
11.	Trinidad	106—3	61—6
12.	Falmouth........................	102—6	80—0
13.	Stony Hill	90—2	70—5
14.	British Guiana	84—0	59—2
15.	Lucea	84—9	63—2
16.	Fort Augusta	73—5	55—5
17.	St. Kits, Nevis and Tortola	71—0	42—1
18.	Grenada	61—8	26—3
19.	Barbadoes	58—5	11—8
20.	St. Vincents.....................	54—9	11—2
21.	Antigua and Montserrat......	40—6	14—9
22.	Maroon Town	32—7	15—3

The most sickly as well as the most fatal period of the year extends from August to December, and during this time the winds are generally from the south and west. The least unhealthy months are March, April, and May, when the trade-wind blows from the east. The annual mortality of the troops in England has for a long series of years been only fifteen to the thousand.

XXI. *On the Colour of Steam under certain circumstances. By* Professor Forbes*.

IN the end of May or beginning of June last, I happened to stand near a locomotive engine on the Greenwich railway, which was discharging a vast quantity of high pressure steam by its safety valve. I chanced to look at the sun through the ascending column of vapour, and was struck by seeing it of a very deep orange red colour, exactly similar to dense smoke, or to the colour imparted to the sun when viewed through a common smoked glass.

I did not pay much attention to the fact at the moment, nor did I attempt to vary the experiment; but reflecting on it af-

* Communicated by the Author.

terwards it seemed to me not only as in itself very singular, but as still more extraordinary that I should never have heard of a property of steam which must have been witnessed by thousands of persons. Some months after (in the end of October), being on the Newcastle and Carlisle railway, I resolved to verify the fact, which I had no difficulty in doing, and I further discovered a very important modification of it. For some feet or yards from the safety valve at which the steam blows, its colour for transmitted light is the deep orange red I have described*. At a greater distance, however, the steam being more fully condensed, the effect entirely ceases: even at moderate thicknesses the steam cloud is absolutely opake to the direct solar rays, the shadow it throws being as black as that of a dense body; and when the thickness is very small it is translucent, but *absolutely* colourless, just like thin clouds passing over the sun, which have indeed a perfect analogy of structure. When the steam is in this state no indication of colour is perceptible in passing from the thickness corresponding to translucency to that which is absolutely opake.

Having made these observations, which were all that the circumstances enabled me to accomplish, I was very anxious to verify them with steam of various pressures, and to determine the following amongst other points: (1) whether steam in its purely gaseous form is really, as commonly supposed, colourless; (2) whether the colour depends on a stage in the process of condensation, and on that alone; (3) what effect the tension of the steam has upon the phænomena.

But there was another inquiry which interested me much more than all these, which was to examine how the spectrum was affected by the absorbent action of the steam, which left the red and orange rays predominant. Judging from the phænomena of absorption of light by gaseous bodies, and especially the singular action of nitrous acid gas in dividing the spectrum into a vast number of bands, discovered by Sir D. Brewster, I thought it by no means improbable that steam acting in a similar manner might exercise its specific action upon the prismatic colours at many points. Should this conjecture be confirmed, I also foresaw an application to the phænomena of the atmosphere and the production of the atmospheric lines of the solar spectrum also remarked by Sir D. Brewster.

After various ineffectual attempts to obtain the requisite fa-

* The same may be observed during the ordinary progress of the engine in the steam thrown into the chimney, but the presence of smoke renders the experiment less satisfactory.

cilities, Mr. Edington of the Phœnix iron-works at Glasgow most kindly put at my disposition an excellent high-pressure boiler, and further afforded me every facility for prosecuting my experiments on the optical properties of steam. I first examined the simple phænomena of colour as seen by the naked eye. A lantern* was held behind a jet of steam issuing from a stopcock in the top of the boiler, having a bore of $\frac{1}{4}$ inch. When the safety valve (which acted with great promptness) was loaded with 50 pounds on the inch, the steam issued nearly invisible, and at the small thickness of the jet in that part perfectly colourless. As the light was raised the orange colour appeared at the height of a few inches above the cock, and rapidly deepened up to a height of about 20 inches, after which it appeared that the rapid condensation of the steam only rendered it more opake without deepening its hue. At that point therefore I resolved to transmit the light and to analyse by a prism. A theodolite and good prism in front of the telescope were placed at the distance of about 25 feet from the boiler; beyond the steam-cock a lantern with a lens for parallel rays was adjusted, and between the steam-cock and the prism a slit of variable width. The light reaching the prism through the slit must first pass through the column of steam at a height of about 20 inches from the orifice. To test the adjustment of the apparatus, and also for the purpose of contrast, I had provided a bottle, about 5 inches in diameter, full of remarkably dense nitrous acid gas, which Mr. Kemp was so good as to prepare for me. When this was placed where the steam was to issue, the appearance of the nitrous acid spectrum was magnificently displayed. I then removed the bottle and opened the steam-cock gradually (the pressure on the safety-valve being 55 pounds above the atmosphere, or the tension of the steam $4\frac{2}{3}$ atmospheres), the violet end of the spectrum was almost instantly absorbed, then the whole blue and part of the green, just as in the nitrous acid spectrum, but *no lines were visible in the remaining part.* When the cock was fully opened the spectrum exhibited a singular appearance; the bright red was the only part which seemed natural. The extreme red was slightly invaded by the opacity of the steam. Most of the orange, the yellow, and as much of the green as was not absorbed had a dirty disagreeable hue, which I described in a memorandum at the time as "dingy, alternating between yellow and purple, with shades of green; when the steam had its highest pressure there was a decidedly purple tinge." The appearance to the naked eye of the slit was now identically the colour of the nitrous acid

* The experiments were performed at night.

gas, through which I from time to time viewed a distant gas flame, and compared it with the colour of the slit. The experiment was performed under 50 and 55 pounds many times over. The light examined was then caused to pass through the steam only 10 inches from the orifice of the stop-cock, under the idea that though the colour there was fainter, possibly there might be a tendency to develope lines in the spectrum. But the experiment being made under the same pressure as before, the effect was similar, only much less intense: the slit had now but a faint tawny colour, and prismatic analysis showed the violet alone absorbed.

Steam blowing off at 25 pounds, the lantern and slit 20 inches above the orifice as at first. To the eye the light appeared as red as under 55 pounds. Mr. Edington observed that the colour was deeper than that of the nitrous acid gas bottle. Neither he nor his assistant ever observed the colour of steam before. Prismatic phænomena as before; only the obscuration not quite so great.

Steam blowing off at 15 pounds. "Evidently redder than the gas bottle: same phænomena of spectrum, but green remains pure throughout, and verges on (bounds immediately with) orange. During the absorption of violet before vanishing (the steam-cock being gradually opened) it assumes a dirty white colour, verging on yellow and purple." A common lamp was viewed through different parts of the column of steam of this pressure, from the orifice up to a height of 5 or 6 feet, and wherever it was not entirely obscured, it appeared of different shades of smoke colour up to an intense tawny orange.

With 7 pounds on the inch. Still visibly red to the eye; prismatic phænomena similar but slighter.

With 4 pounds, no longer visibly red to the eye when arranged as above; and seen with the prism the violet appears but little affected. When let off in large quantity from the safety-valve, and a lamp viewed through it, there is a faint redness close to the orifice, but everywhere above, the transition is from colourless translucency to complete opacity. At about 2 and 1 pound no colour can be detected.

From these experiments I deduce the following conclusions:

(1.) Steam in its purely gaseous form is, as commonly supposed, colourless, at least at small thicknesses.

(2.) The orange red colour of steam by transmitted light appears to be due to a particular stage of the condensing process. Before condensation, steam is colourless and transparent; it is next transparent and smoke-coloured; finally it

becomes colourless at small thicknesses, and absolutely opake at greater.

(3.) The state of tension of the steam seems only to affect the phænomena so far as it renders the critical colorific stage of condensation more or less completely observable.

(4.) The absorptive action of steam on the spectrum is not exerted in the same way as that of other gaseous coloured bodies, such as nitrous acid gas, and iodine vapour. It cuts off, however, totally the same part of the spectrum as nitrous acid does. Its phænomena perhaps have a greater analogy to those of opalescence than any other.

The effect of mere change of mechanical structure in altering the optical properties of bodies is a phænomenon likely to give important information, both as to the constitution of matter and the constitution of light; and the present observations may perhaps be one day viewed as a contribution towards a mechanical theory of vapour, including that most singular stage which intervenes between the gaseous and completely liquid form, and which is probably connected with the mechanical suspension of clouds. It is at all events very important to know that a portion of watery vapour confined in a close vessel, and subjected to change of temperature alone, without chemical change, is capable of undergoing the alterations of colour and transparency which have been adverted to. The singular fact noticed by Sir D. Brewster in the case of nitrous acid gas, the colour of which deepens to an intense orange red by the simple application of heat, seems to be a fact of this kind.

I cannot doubt that the colour of watery vapour under certain circumstances is the principal or only cause of the red colour observed in clouds. The very fact that that colour chiefly appears in the presence of clouds, is a sufficient refutation of the only explanation of the phænomena of sunset and sunrise having the least plausibility, given by optical writers. If the red light of the horizontal sky were simply complementary to the blue of a pure atmosphere, the sun ought to set red in the clearest weather, and then most of all; but experience shows that a lurid sunrise or sunset is *always* accompanied by clouds, or diffused vapours, and in a great majority of cases occurs when the changing state of previously transparent and colourless vapour may be inferred from the succeeding rain. In like manner terrestrial lights seen at a distance grow red and dim when the atmosphere is filled with vapour soon to be precipitated. Analogy applied to the preceding observations would certainly conduct to a solution of such appearances; for I have remarked that the existence of vapour of

high tension is by no means essential to the production of colour, though of course a proportionally greater thickness of the medium must be employed to produce a similar effect when the elasticity is small.

Glasgow, Dec. 29, 1838.

XXII. *Remarks on a paper in the* Philosophical Magazine *for December* 1838, *on a certain demonstration of Euclid. By* A Correspondent.

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

IN this month's number of the Philosophical Magazine (Dec. 1838. vol. xiii. p. 434) one of your correspondents has given a method of dispensing with the 12th axiom of the 1st book of Euclid's Elements, apparently not being aware that Professor Peacock has given the same in his treatise on algebra. It would, indeed, be immediately suggested by a perusal of the demonstration of the 12th axiom, and the remarks upon it by Ptolemy, quoted by Proclus in his commentary on Euclid's Elements.

It appears to me to be open to the objection which Proclus makes to the emendations of Æneas Hierapolites, and others on the Elements, "for the geometrician appears to have chosen such hypotheses as either abound in affirmation or are more simple." Setting aside other more weighty objections, Professor Peacock's proposed amendment, although it removes one difficulty, is certainly not sufficiently simple to be placed among the definitions of the 1st book. When I say not sufficiently simple, I mean that it does not immediately impress upon one's mind the common idea of the subject defined, an object to which Euclid has always been careful to adhere.

Your obedient Servant,

Jesus College, Cambridge, Dec. 1st, 1238. J. O. H.

XXIII. *Notice of the Electrical Excitation of a Leather Strap connecting the Drums of a Worsted Mill; in a letter to* Dr. Faraday *from the* Rev. T. Drury*.

My Dear Sir,

PERMIT me to describe an extraordinary electrifying machine which I yesterday witnessed, and which I think will be new *even to you.*

* Communicated by Dr. Faraday.

It is no other than a leather strap, which connects two drums in a large worsted mill in the town of Keighley.

The dimensions and particulars of the strap are as follows:

It is in length	24 feet
Breadth.	6 inches
Thickness.	$\frac{1}{8}$ do.

It makes 100 revolutions in a minute.

The drums, over which it passes at both ends, are two feet in diameter, made of wood fastened to iron hoops and turning on iron axles; these drums are placed at 10 feet distance from each other, and the strap crosses in the middle between the drums, where there is some friction; the strap forming a figure of eight. There is no metal in connexion with the strap, but it is oiled. If you present your knuckle to the strap above the point of crossing, brushes of electrical light are given off in abundance, and when the points of a prime conductor are held near the strap, most pungent sparks are given off to a knuckle at about two inches; I charged a Leyden jar of considerable size in a few seconds by presenting it to the prime conductor. The gentleman who told me of this curious strap has frequently charged his electrical battery in a very short time from it, and he informed me that it is always the same, generating electricity from morning to night without any abatement or alteration. If this strap had the advantage of silk flaps and a little amalgam, it would rival the machine in the lecture room in Albemarle-street.

Pray excuse the earnestness of

Your most faithful Servant,

Keighley Rectory, Yorkshire, THEODORE DRURY.
Dec. 17, 1838.

XXIV. *On Voltaic Series and the Combination of Gases by Platinum.* *By* W. R. GROVE, *Esq.* M.A.

GENTLEMEN, Swansea, Dec. 14, 1838.

IN a letter on an œconomical constant battery which you did me the honour to publish in your number for the present month, (Dec. 1838. vol. xiii. p. 430) I ventured to suggest the more extensive employment of the porous septum as an instrument of analysis for voltaic combinations. I am not unaware of the experiments of De la Rive, Porret, &c., and meant to allude less to its use in the decomposing cell, than in the trough itself, and to its practical application to the improvement of apparatus. The following experiments instituted with this view may not be uninteresting to your readers; they differ, it will

be seen, materially from those of Sir H. Davy on unimetal series. Having constructed two troughs in the manner described in my last letter, one of alternate plates of iron and unglazed porcelain, the other of plates of copper and porcelain, I poured into the alternate cells of the first a saturated solution of sulphate of iron and dilute sulphuric acid. With this arrangement, as was to have been expected, little electric action was manifest; equally trifling were the effects when sulphate of iron and dilute muriatic acid were the electrolytes; when however nitric acid was employed with sulphate of iron a tolerably active current was generated: with twelve plates acidulated water was decomposed and a slight shock felt in the moistened hands. I now tried the copper trough with sulphate of copper and the same three acids respectively: with the sulphuric and nitric the electric development was but slight; but with the muriatic, diluted with about twice its quantity of water, a most energetic series was formed. With twelve plates acidulated water was rapidly decomposed*: with a pair of copper plates each exposing about 36 square inches of surface, a Ritchie's rotating magnet was whirled rapidly round, exhibiting small but brilliant sparks; its revolution continued for several hours without the addition of fresh acid; in fact the energy was fully equal to that displayed by similarly sized arrangements of zinc and copper, excited by muriatic acid but without diaphragm: a strong solution of common salt, substituted for muriatic acid, produced effects not far inferior. On examining the batteries when exhausted, I found the sides of the copper which had been exposed to the sulphate of copper covered with a fine coating of that metal; the affinity between the chlorine and the copper had consequently (according to the principle of preponderating affinity established by Dr. Faraday,) been sufficiently powerful to cause the solution of copper to be de-oxidated by the transferred hydrogen and to produce vigorous electro-motive action without the presence of a dissimilar metal.

It would appear from this that the diaphragm is of more practical importance in voltaic combinations than as a mere

* It is more expensive but much more satisfactory, if, in these experiments, series, as in the text, be employed instead of single combinations. I have frequently imagined I had obtained results from a single pair with the galvanometer, but have found them entirely negatived when the same combination was used in series. This was most probably owing to the many interfering circumstances to which the magnetic galvanoscope is liable, or perhaps to superficial differences in the two plates of metal.

preventer of cross precipitation; for instance, if zinc and copper be employed with muriatic acid but without diaphragm, putting out of the question the precipitation of the zinc on the copper, the power would be only as the excess of the affinity of chlorine for zinc over its affinity for copper; with the diaphragm we have no opposing current, the affinity of chlorine for copper, assisted by that of hydrogen for oxygen, is able readily to cause decomposition of the sulphate of copper and give rise to a strong current. In the first or common arrangement, this current opposes, and consequently, in estimating the resulting power, must be deducted from that produced by the superior affinity of chlorine for zinc; in the last arrangement, the thus evidently inferior obstacle, the resistance to decomposition of the sulphate, is the only one to be overcome*.

It would seem then that the best form of combination would be one with two metals and two electrolytes, the generating metal being one which has the strongest affinity for the anion of the electrolyte in contact with it, while the other solution is most readily decomposable by its cation and does not cause a precipitate upon which its own anion would readily react; zinc with muriatic acid and copper with sulphate of copper fulfil these conditions to a great degree; if these principles be correct, very superior combinations may be discovered. I cannot refrain from expressing, with much diffidence, a hope that these experiments may be thought worthy of verification and extension by those "older in practice, abler than myself."

I remain, Gentlemen, yours, &c.,

W. R. Grove.

P.S. Jan. 1839. I should have pursued these experiments further, and with other metals, but was led aside by some experiments with different solutions separated by a diaphragm and connected by platinum plates; in many of these I have been anticipated.

I will however mention one which goes a step further than any hitherto recorded; and affords, I think, an important illustration of the combination of gases by platinum.

Two strips of platinum 2 inches long and three-eighths of an inch wide, standing erect at a short distance from each

* The reason why iron with sulphate of iron and muriatic acid is inferior to the copper combination here described, may be that the difference of affinities is not so great, but more probably proceeds from the minute currents on the surface of the iron weakening the efficacy of the chemical action to produce a general current; copper being more homogeneous evolves no hydrogen and the whole action is utilized. Copper with sulphuret of potassium and sulphate of copper is a most powerful unimetal combination, if unimetal it may be called.

other, passed, hermetically sealed, through the bottom of a bell glass; the projecting ends were made to communicate with a delicate galvanometer; the glass was filled with water acidulated with sulphuric acid, and both the platina strips made the positive electrodes of a voltaic battery until perfectly clean, &c.; contact with the battery having been broken, over each piece of platinum was inverted a tube of gas, four-tenths of an inch in diameter, one of oxygen, the other of hydrogen, acidulated water reaching a certain mark on the glass, so that about half of the platina was exposed to the gas, and half to the water. The instant the tubes were lowered so as to expose part of the surfaces of platinum to the gases, the galvanometer needle was deflected so strongly as to turn more than half round: it remained stationary at 15°, the platinum in the hydrogen being similar to the zinc element of the pile. When the tubes were raised so as to cover the plates with water, the needle returned slowly to zero; but the instant that the tubes were lowered again, it was again deflected; if the tubes were changed with regard to the platina, the deflection was to the contrary side.

The action lowered considerably after the first few minutes, but was in some degree restored every time the tubes were raised so as to wash the surface of the platina, and again lowered. After 24 hours, the water had risen half an inch in the tube containing hydrogen, and three eighths of an inch in that containing oxygen. In two other tubes, without platina, but with the same gases and immersed in acidulated water for the same time, the water had scarcely perceptibly risen, the effect therefore could not have been due to solution; the same sheets of platinum were exposed to atmospheres of common air and of similar gases, i. e. both to oxygen or both to hydrogen, &c., but without affecting the galvanometer. The platinum in the hydrogen was made the positive, and that in the oxygen the negative electrode of a *single* voltaic pair; the water now rose at the rate of three-eighths of an inch per hour in the hydrogen tube and proportionally in the oxygen; when the platina was not assisted by a pair of metals the oxygen was absorbed in more than its relative proportion. I hope, by repeating this experiment in series, to effect decomposition of water by means of its composition.

XXV. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

Anniversary Meeting, Nov. 30, 1838 (*continued*).—The following Report of the Council respecting the awards they have made of two Copley Medals, two Royal Medals, and one Rumford Medal, was read.

The Council have awarded a Copley Medal to Professor Gauss, for his researches and mathematical researches on Magnetism.

Professor Gauss's labours on the subject of magnetism, published at various periods, and continued with increasing activity up to the present time, have given to our knowledge of that subject very valuable and striking additions. In his dissertation entitled, "Intensitas vis magneticæ terrestris ad mensuram absolutam revocata*," (Göttingen, 1833,) he showed how, by a skilful combination of experiment with mathematical calculation, several of the most difficult problems belonging to the subject may be solved; namely, the determination of the magnetic axis of a needle; the exact determination of the moment of inertia of an oscillating needle; the deviation produced in the direction of the horizontal needle by the neighbourhood of a magnet; and the determination of the absolute intensity of the horizontal magnetic force of the earth. A combination of magnetic observers in different places had been set on foot by M. von Humboldt in 1828; a magnetic observatory was erected at Göttingen in 1833; and in consequence of these circumstances the curious discovery was made in 1834, that the minute momentary changes in the position of the horizontal needle are simultaneous and corresponding at distant places. This led M. Gauss to direct the attention of men of science more particularly to this subject; and the operations of the "Magnetic Union" of observers were carried on with great activity under his guidance. The "Results of the observations of the Magnetic Union" for 1836 and for 1837, published by MM. Gauss and W. Weber, contain an account of the consequences of these exertions. They also contain descriptions of instruments invented by M. Gauss for the purpose of these observations, namely, the *magnetometer*, and other magnetical apparatus of his construction, which has already been sent to the observatories of Bonn, Dublin, Freiberg, Greenwich, Kasan, Milan, Munich, Naples, Upsala, Krakow, Leipzig, and Marpurg. Also the *Bifilar Magnetometer*, which determines directly the variation of horizontal intensity. The "Results" further contain various mathematical calculations of great importance, on the subject of the above instruments, and of the observations made by them. And it appears by observations made in March, 1838, at Göttingen and three other places, with the Bifilar apparatus, that there is the same correspond-

* An abstract of Prof. Gauss's dissertation was given in Lond. and Edinb. Phil. Mag., vol. ii. p. 291; and a translation of the "Results" published by him and Weber, also mentioned in the above report of the Council, is preparing for immediate publication in the Fifth Number of the Scientific Memoirs.—Edit.

ence in the simultaneous changes of intensity at different places which had already been discovered in the declination. The ingenuity shown in the invention of instruments and processes, the mathematical skill employed in treating the observations, and the importance and interest of the results, are well deserving of being honourably marked by the Royal Society, and the adjudication of the Copley Medal to M. Gauss.

The Council have also awarded a Copley Medal to Dr. Faraday for his discovery of Specific Electrical Induction, published in the eleventh series of his Experimental Researches in Electricity*.

From the peculiar view which he had taken of the phenomena of induction, Dr. Faraday was led to expect some particular relation of this process to different kinds of matter, through which it might be exerted. This relation he succeeded in establishing by the most decisive experiments.

The phenomena are shown in their simplest form by an instrument which he has named a Differential Inductometer. It consists of three insulated metallic plates, placed facing each other; the centre one being fixed, and the other two moveable upon slides, by which they may be approximated to or withdrawn from the centre. Each end plate is connected with an insulated leaf of an electrometer. When a charge is communicated to the centre plate under ordinary circumstances, the induction is equal on both sides, and the gold leaves are not disturbed. But if after uninsulating them, and again insulating them, a thick plate of shell-lac or sulphur be interposed between two of the plates, unequal induction will take place on the two sides, and the gold leaves will attract one another. By these means Dr. Faraday ascertained that, taking the specific inductive capacity of air to be . . . . 1·

That of Glass is	1·76
Shell-lac	2·
Sulphur	2·24

The results obtained with spermaceti, oil of turpentine, and naphtha were higher than that of air, but their conducting powers interfered with the accuracy of the experiments.

By another form of apparatus he ascertained that all aëriform matter has the same power of sustaining induction; and that no variations in the density or elasticity of gases produced any variation in their electric tension until rarefaction is pushed so far as that discharge may take place across them.

Hot and cold air were compared together, and damp and dry air, but no difference was found in the results.

The great importance of the discovery and complete establishment of such a principle as that of specific inductive capacity, in all its relations both experimental and theoretic, is so palpable, that any comment must be superfluous; and the Council have felt they can-

* Dr. Faraday's Eleventh Series of Researches will be found in our last volume, p. 281, *et seq.*, and the Supplement to it in the preceding Number, p. 34.

not better mark their sense of the value of this discovery than by awarding the Copley Medal to its author.

The Council have awarded the Royal Medal for Mathematics to H. F. Talbot, Esq., for his two memoirs entitled, "Researches in the Integral Calculus," published in the Philosophical Transactions for 1836 and 1837*.

Nothing perhaps tends more directly to bring the correctness of refined theoretical investigations in physics to the test of numerical results, than improvements in and extensions of the processes of integration. Any advance therefore which is made in this difficult branch of analysis must be viewed not merely in the light of a difficulty overcome in the progress of abstract science, but likewise as having an important bearing on the advancement of physical inquiry.

The branch of analysis to which Mr. Talbot's researches belong is one which is connected with a long series of valuable investigations from the time of Fagnani and Euler to that of Legendre, Jacobi, and Abel: it relates to integrals under the same form which are separately nonscendental, but which furnish, under particular conditions of the variables, an algebraical result when two or more of them are connected together with the signs + or —. The celebrated theorem of Abel†, which may be made to comprehend some of Mr. Talbot's results, is the most comprehensive and most important of all the general conclusions which have been arrived at in this department of analysis: but the process adopted by Mr. Talbot is more allied to that followed by Fagnani than by Abel, and is equally remarkable for its great simplicity and for the vast number of novel and interesting results which it furnishes, including not merely several of the most remarkable of those which are already known, but likewise many others which are apparently not deducible by other methods.

The Council have awarded the Royal Medal for Chemistry to Professor Thomas Graham for his paper entitled "Inquiries respecting the Constitution of Salts; of Oxalates, Nitrates, Phosphates, Sulphates, and Chlorides," which was read to the Society on the 24th of November 1836‡, and since published in the Philosophical Transactions. This paper they have considered as being the last of a series on a general subject of great importance: and as the sequel of Professor Graham's researches on the Arseniates, Phosphates, and modifications of Phosphoric Acid, read to the Society on the 19th of June 1833, and published in the Philosophical Transactions of the same year‡. He has therein shown that, by considering the water which enters into the composition of the different classes of salts, which the phosphoric acid forms with the several bases, and which has been considered as water of crystallization as standing in a *basic* relation to the acid, a very simple view might be taken of

* Abstracts of Mr. Talbot's papers appeared in Lond. and Edinb. Phil. Mag., vol. viii. p. 549, and vol. xi. p. 210.—Edit.

† See Lond. and Edinb. Phil. Mag., vol. vi. p. 116.—Edit.

‡ An abstract of Prof. Graham's papers appeared in Lond. and Edinb. Phil. Mag., vol. iii. p. 451, 459, and vol. x. p. 216.—Edit.

this very complicated subject. According to this theory, there are three sets of phosphates, in which the oxygen of the acid being 5, the oxygen in the base is respectively 3, 2, or 1; the remaining equivalents of oxygen, in the two first cases, being supplied by that portion which exists in the 2 or 3 equivalents, respectively, of the basic water, which water is wholly absent in the third case. These three classes of salts Professor Graham proposes to term, respectively, *monobasic*, *bibasic*, and *tribasic* salts. Professor Graham has extended these views of the basic formation of water in salts to the case of the sulphates, in a paper communicated to the Royal Society of Edinburgh, and published in the 13th volume of their Transactions, on "Water as a constituent of Salts*." The principal object of this paper, however, was to show that water exists in a different state in certain salts, and does not exercise a true basic function, being capable of being replaced by a *salt*, and not by an *alkaline base*, and giving rise to a class of *double salts*. This inquiry was suggested by the tendency of phosphate of soda to unite with an additional dose of soda, and form a *subsalt*, which had been traced to the existence of basic water in the former. The result was, that in the well-known class of sulphates, consisting of sulphates of magnesia, zinc, iron, manganese, copper, nickel and cobalt, all of which crystallize with either five or seven equivalents of water, one equivalent proved to be much more strongly united to the salt than the other four or six. The latter, to which the name of *water of crystallization* should be restricted, may generally be expelled by a heat under the boiling point of water; while the remaining equivalent uniformly requires a heat above 400° of Fahrenheit for its expulsion, and seems to be, in a manner, essential to the salt. Thus in the double sulphate of zinc and potassa, the single equivalent of water, existing in the sulphate of zinc, is replaced by an equivalent of sulphate of potassa, while the six equivalents of water of crystallization remain; and all the other salts of this class combine with one another in a similar manner.

The super-sulphates must also be regarded as analogous to double salts; the bisulphate of potassa, for example, being a sulphate of water and potassa.

There is likewise a provision in the constitution of hydriated sulphuric acid for the production of a double salt analogous in its constitution to sulphate of zinc. Sulphuric acid, of the specific gravity 1·78, contains two equivalents of water, and is capable of crystallizing at a temperature of 40° of Fahrenheit, being, in fact, the only known crystallizable hydrate of sulphuric acid. The second equivalent of water, contained in the hydrated acid, is capable of being replaced by an equivalent of sulphate of potassa, which is itself a salt, and a bisulphate of potassa is the result of this substitution. But the first equivalent of water can be replaced only by an alkali or true base. Professor Graham distinguishes water in these two

* The paper here referred to will be found also in Lond. and Edinb. Phil. Mag., vol. vi. p. 327 *et seq.*—Edit.

states of combination as *basic* and *saline* water. Thus the hydrate of sulphuric acid, already mentioned, contains one equivalent of basic, and one equivalent of saline water. It is, in his nomenclature, a *sulphate of water with saline water*, as the hydrous sulphate of zinc is a *sulphate of zinc with saline water*. The bi-sulphate of potassa is also a *sulphate of water with sulphate of potassa*, and corresponds with the double salt of sulphate of zinc with sulphate of potassa.

The results which Professor Graham has thus obtained, and which he has communicated, partly to the Royal Society, and partly to the Royal Society of Edinburgh, suggested to him the probability that the law with respect to water in the constitution of the sulphates would extend to any hydrated acid, and the magnesian salt of that acid; and his researches on this extension of the subject constitute the substance of his last communication to the Royal Society. As he had already found that the sulphate of water is constituted like the sulphate of magnesia, so he now finds oxalate of water to resemble the oxalate of magnesia, and the nitrate of water to resemble the nitrate of magnesia. He is moreover of opinion, that this correspondence between water and the magnesian class of oxides extends beyond their character as bases, and that, in certain subsalts of this class, the metallic oxide replaces the water of crystallization of the neutral salt, and discharges a function which was thought peculiar to water.

The same kind of displacement, which takes place in the formation of a double sulphate by the substitution of a salt of the same class for an equivalent of water, appears to occur likewise in the constitution of double oxalates; and the application of this principle elucidates the constitution of that class of salts, as well as of the super-oxalates, and explains the mode in which they are derived.

Lastly, the same law is traced in the constitution of the chlorides of the magnesian class of metals, which are found to have two equivalents of water strongly attached to them, and which may therefore be considered as constitutional. Many of them have two or four equivalents more, the proportion advancing by multiples of two equivalents.

Professor Graham has supported these views, not only by numerous arguments, but also by experimental investigations of the physical properties of different classes of salts, and a great number of chemical analyses; and he has thus largely added to our positive knowledge of this somewhat neglected branch of chemical science.

The Council, without pronouncing any judgement on the question whether Professor Graham's hypothesis concerning the different functions of water in the constitution of salts be a representation of the real mechanism of nature, are of opinion, that the discussion of his new and ingenious views will be highly conducive to the progress of science, particularly in the department of organic chemistry, in which they have been already followed out with success by some eminent foreign chemists, and have accordingly awarded to Professor Graham the Royal Medal for Chemistry of the present year, for his valuable researches in this department of science.

The Council have awarded the Rumford Medal to Professor Forbes, for his discoveries and investigations of the Polarization and Double Refraction of Heat, published in the recent volumes of the Transactions of the Royal Society of Edinburgh*.

That solar heat, like the light which it accompanies, may be polarized, was shown by the early experiments of MM. Malus and Berard; but the announcement of M. Berard, that heat from other sources was also capable of polarization, not having been confirmed in subsequent repetitions of his experiments by other philosophers, it became of the highest importance to establish this analogy between light and heat from whatever source the latter might be derived.

The admirable instrument, the Thermo-multiplier, invented by MM. Nobili and Melloni, afforded facilities for the prosecution of inquiries of this nature, of which the inventors and others were not tardy in availing themselves. One of the most important results obtained by M. Melloni, and confirmed by Professor Forbes, the refrangibility of non-luminous heat by a prism of rock-salt, appeared to point to the polarization and double refraction of heat as almost necessary consequences. The experiments, however, of both these philosophers with tourmaline, undertaken nearly at the same time, appeared to negative the fact; but Professor Forbes becoming sensible of the source of error, in the conclusions he had at first drawn from his experiments, soon saw that his results clearly indicated the effect he was in search of. His subsequent experiments established the fact, that in the transmission of heat from an Argand lamp, from incandescent platinum, and even from non-luminous heated brass, through slices of tourmaline cut parallel to the axis of the crystal, a portion of the heat is polarized, when the axes of the crystals are at right angles to each other; and these results were confirmed by M. Melloni.

But Professor Forbes did not allow the polarization of heat to rest solely upon the results obtained with tourmaline. By employing bundles of plates of mica, and adjusting them at proper angles, he not only obtained much more decisive results, particularly with heat from a non-luminous source, but such results as go to establish the singular fact, that the degree of the polarization of heat is dependent on the nature of its source. He has further shown the depolarization of heat by the interposition of a mica plate, and its circular polarization by means of two total internal reflections in an interposed rhomb, or two prisms of rock-salt.

The Council consider that they cannot better testify their estimation of the discoveries and experimental investigations of Professor Forbes, than by awarding to him a Medal, bequeathed by its distinguished founder, as a premium to the author of discoveries tending to improve the theories of heat and light.

* See Lond. and Edinb. Phil. Mag., vol. vi. p. 134 *et seq.*; vol. xi. p. 542; vol. xii. p. 545. The papers in which M. Melloni's Researches are detailed have been given in the first volume of the Scientific Memoirs.—Edit.

President.—The Marquis of Northampton.—*Treasurer.*—John William Lubbock, Esq., M.A., V.P.—*Secretaries.*—Peter Mark Roget, M.D.; Samuel Hunter Christie, Esq., M.A.—*Foreign Secretary.*—William Henry Smyth, Capt. R.N.—*Other Members of the Council.*—H.R.H. the Duke of Sussex, K.G., V.P.; Francis Baily, Esq., V.P.; John George Children, Esq., V.P.; John Frederic Daniell, Esq.; C. G. B. Daubeny, M.D.; Thomas Galloway, Esq., M.A.; Thomas Graham, Esq.; Sir John F. W. Herschel, Bart., M.A., V.P.; Francis Kiernan, Esq.; George Rennie, Esq.; John Forbes Royle, M.D., V.P.; Rev. Adam Sedgwick, M.A.; Robert Bentley Todd, M.D.; Charles Wheatstone, Esq.; Rev. William Whewell, M.A.; Rev. Robert Willis, M.A.

Report of a Joint Committee of Physics and Meteorology referred to, by the Council of the Royal Society, for an opinion on the propriety of recommending the establishment of fixed magnetic observatories, and the equipment of a naval expedition for magnetic observations in the Antarctic Seas, to Her Majesty's Government, and to report generally on the subject: together with the Resolutions adopted on that Report, by the Council of the Royal Society.

Report.—The subject of terrestrial magnetism has recently received some very important accessions which have materially affected not only the point of view in which henceforward it will be theoretically contemplated, but also the modes of observation which will require to be adopted for completing our knowledge of the actual state of the magnetic phænomena, and furnishing accurate data for the construction and verification of theoretical systems. It was for a long time supposed that the changes in the position assumed by the needle at any particular point on the earth's surface might be conceived as resulting from regular laws of periodicity, having for their arguments, 1st, a great magnetic cycle of several centuries, depending on unknown, and perhaps internal movements or relations; and 2ndly, on the periodic alternations of heat and cold depending on the annual and diurnal movements of the sun. The discovery of the affection of the needle by the aurora borealis, and of the existence of minute and irregular movements, which might be referred either to unperceived auroras or to other local and temporary causes, sufficed to show that the laws of terrestrial magnetism are not so simple as to admit of this summary form of expression; and the important discovery, first announced, we believe, by Baron Von Humboldt, that those temporary changes take place simultaneously at great distances in point of locality, a discovery which has since been remarkably confirmed and extended to very great intervals of distance, so as to include the whole extent of the European continent, by Gauss and Weber, and their coadjutors of the German Magnetic Association, has sufficed to show that the gist of the inquiry lies deeper, and depends upon relations far more complex, while at the same time the dominion of what might previously have been regarded as local agency, would require, in the new views consequent on the establishment of these facts, to be extended far beyond what ordinary usage would authorize as a just application of that epithet.

For a long time in the history of terrestrial magnetism the variation alone was attended to. The consideration of the dip was then superadded; but the observation of this element being more difficult and delicate, our knowledge of the actual and past state of the dip over the earth's surface is lamentably deficient. It has lately appeared, however, that this element can be observed with considerable approximation, though not with nicety, at sea, so that no reason subsists why materials for a chart of the dip analogous to that of variation should not be systematically collected. Lastly, the intensity has come to be added to the list of observanda; and from the great facility and exactness with which it can be determined, this branch of magnetic knowledge has in fact made most rapid progress.

These three elements, the Horizontal Direction, the Dip, and the Intensity, require to be precisely ascertained before the magnetic state of any given station on the globe can be said to be fully determined. Nor can either of them, theoretically speaking, be said to be more important than the others, though the direction, on account of its immediate use to navigators, has hitherto had the greatest stress laid upon it, and been reduced into elaborate charts. A chart of the lines of total intensity has been recently constructed by Major Sabine.

All these elements are, at each point, now ascertained to be in a constant state of fluctuation, and affected by those transient and irregular changes which are above alluded to; and the investigation of the laws, extent, and mutual relations of these changes is now become essential to the successful prosecution of magnetic discovery, for the following reasons.

1st. That the progressive and periodical being mixed up with the transitory changes, it is impossible to separate them so as to obtain a correct knowledge and analysis of the former, without taking express account of and eliminating the latter, any more than it would be practicable to obtain measures of the sea-level available for an inquiry into the tides, without destroying the irregular fluctuation produced by waves.

2ndly. That the secular magnetic changes cannot be concluded from comparatively short series of observations without giving to those observations extreme nicety, so as to determine with perfect precision the mean state of the elements at the two extremes of the period embraced, which, as already observed, presupposes a knowledge of the casual deviations.

3rdly. It seems very probable that discordances found to exist between results obtained by different observers, or by the same at different times, may be, in fact, *not* owing to error of observation, but may be due to the influence of these transitory fluctuations in the elements themselves.

4thly and lastly. Because the theory of these transitory changes is in itself one of the most interesting and important points to which the attention of magnetic inquirers can be turned, as they are no doubt intimately connected with the general causes of terrestrial magnetism, and will probably lead us to a much more perfect knowledge of those causes than we now possess.

Actuated by these impressions, on the occasion of a letter addressed by Baron Von Humboldt to His Royal Highness the Duke of Sussex, P.R.S*., the Council of this Society, on April 13, 1837, resolved to apply to Government for aid in prosecuting, in conjunction with the German Magnetic Association, a series of simultaneous observations; and in consequence of an application founded on such their resolution, a grant of money was obtained for the purchase of instruments for that purpose. By reason, however, of the details and manipulations of the methods then recently introduced into magnetic observations by Gauss being at that time neither completely perfected, nor their superiority over the old methods fully established by general practice, the precise apparatus to be employed in these operations was not at the time agreed upon, and was still under discussion, subject to the report of the Astronomer Royal on the performance of an instrument on Gauss's principle established at Greenwich, at the time when the subject in its present more extended form was referred by the Council to this Joint Committee, so that the grant in question has not, in point of fact, been employed or called for. The Committee consider this as in some respects fortunate, as in consequence of the delay time has been given for a much maturer consideration of the whole subject; and should it now be taken up as a matter of public concern, they consider that it will be necessary to provide for a more continuous and systematic series of observations, by observers regularly appointed for the purpose, and provided with instruments and means considerably more costly than those contemplated on the occasion in question.

On the general advisableness of calling for public assistance in the prosecution of the extensive subject of terrestrial magnetism, in both the modes referred to them for their consideration, (viz. by magnetic observatories established at several stations properly selected on land, and by a naval expedition expressly directed to such observations in the Antarctic Seas,) your Committee are fully agreed. They consider the subject to have now attained a degree of theoretical as well as of practical importance, and to afford a scope for the application of exact inquiry which it has never before enjoyed, and which are such as fully to justify its recommendation by the Royal Society to a revival of that national support to which we are indebted for the first chart of variations constructed by our illustrious countryman Halley in A.D. 1701, on the basis of observations collected in a voyage of discovery expressly equipped for that purpose by the British Government.

As regards the first branch of the question referred to their consideration, they are of opinion that the stations which have been suggested to them, viz. Canada, St. Helena, the Cape, Van Diemen's Land, and Ceylon (or Madras), are well selected, and perhaps as numerous as they could venture to recommend, considering the expense which would require to be incurred at each, and that in each of

* A translation of Baron Von Humboldt's letter was given in Lond. and Edinb. Phil. Mag. vol. ix. p. 42.—Edit.

these stations it would be desirable, 1st, That regular hourly observations should be made (at least during the daytime) of the fluctuations of the three elements of variation, dip, and intensity, or their equivalents, with magnetometers on the more improved construction, during a period of three years from their commencement.

2ndly. That on days, and on a plan appointed, agreed on in concert with one another, and with European observatories, the fluctuations of the same elements should be observed during twenty-four successive hours, strictly simultaneous with one another, and at intervals of not more than five minutes.

3rdly. That the absolute values of the same elements shall be determined at each station, in reference to the fluctuating values above mentioned, with all possible care and precision, at several epochs comprehended within the period allowed.

4thly. That in the event of a naval expedition of magnetic discovery being despatched, observations be also instituted at each fixed station, in correspondence with, and on a plan concerted with, the Commander of such Expedition.

As regards the second branch of the subject referred to them, viz. the proposal of an Antarctic voyage of magnetic research, they are of opinion, as already generally expressed, that such a voyage would be, in the present state of the subject, productive of results of the highest importance and value; and they ground this opinion on the following reasons:—

1st. That great and notorious deficiencies exist in our knowledge of the course of the variation lines generally, but especially in the Antarctic seas, and that the true position of the southern magnetic pole or poles can scarcely even be conjectured with any probability from the data already known.

2ndly. That our knowledge of the dip throughout those regions, and the whole southern hemisphere, is even yet more defective, and that even such observations of this element as could be procured at sea, still more by landing on ice, &c., would have especial value.

3rdly. That the intensity lines in those regions rest on observations far too few to justify any sure reliance on their courses over a large part of their extent, and over the rest are altogether conjectural. Nevertheless that there is good reason to believe in the existence and accessibility of two points of maximum intensity in the southern as in the northern hemisphere, the attainment of which would be highly interesting and important.

4thly. That a correct knowledge of the courses of these lines, especially where they approach their respective poles, is to be regarded as a first and, indeed, indispensable preliminary step to the construction of a rigorous and complete theory of terrestrial magnetism.

5thly. That during the progress of such an expedition, opportunities would of necessity occur (and should be expressly sought) to observe the transitory fluctuations of the magnetic elements in simultaneous conjunction with observations at the fixed stations and in Europe, and so to furnish data for the investigation of these

changes in localities very unlikely to be revisited for any purposes except those connected with scientific inquiries.

Your Committee, in making this Report, think it unnecessary to go into any minute details relative to the instruments or other materiel required for the proposed operations, still less into those of the conduct of the operations themselves. Should such be required from them, it will then be time to enter further into these and other points, when the Committee will most readily devote themselves to the fullest consideration of the subject.

J. F. W. Herschel,
Chairman of the Joint Physical and Meteorological Committee.

Resolutions.—1. That this Report be received and approved.

2. That the Council, deeply impressed with the importance of the scientific objects which might be attained by an Antarctic Expedition, particularly by the institution of magnetic observations in southern regions, do earnestly recommend that Her Majesty's Government be pleased to direct the equipment of such an expedition.

3. That the imperfect state of our present knowledge of the amount and fluctuations of the magnetic elements, renders the establishment of fixed magnetical observatories, for a limited time, at various points of the earth's surface, highly desirable, particularly in Canada, St. Helena, Van Diemen's Land and Ceylon, and at the Cape of Good Hope, and that the Council do earnestly recommend Her Majesty's Government to cause such observatories to be established.

4. That a deputation, consisting of the President, Treasurer, and Secretaries of the Society, Sir John F. W. Herschel, the Chairman, and Major Sabine and Mr. Wheatstone, the Secretaries of the joint Committee of Physics and Meteorology, be requested to communicate the above Resolutions to Lord Melbourne, and to urge on the Government the adoption of the measures therein proposed.

GEOLOGICAL SOCIETY.

Nov. 21, 1838.—A paper was first read "On the Jaws of the *Thylacotherium Prevostii** (Valenciennes) from Stonesfield," by Richard Owen, Esq., F.G.S., Hunterian Professor, Royal College of Surgeons.

Doubts having been recently expressed by M. de Blainville†, from inspection of casts, respecting the mammiferous nature of the fossil jaws found at Stonesfield, and assigned to the Marsupialia by Baron Cuvier, Mr. Owen brought the paper before the Society, to meet the objections and give a detailed account of the fossils from a careful inspection of the originals. In this communication, however, he confined his description chiefly to the jaws of one of the two genera which have been discovered at Stonesfield, and characterized by

* Comptes Rendus, 1838; Second Semestre, No. 11, Sept. 10, p. 580.

† Ibid., No. 8, Août 20, p. 402 *et seq.*; No. 9, Planche; No. 17, Oct. 22, p. 727; No. 18, Oct. 29, p. 750.

having eleven molars in each ramus of the lower jaw, reserving to a future occasion an account of the remains of the other genus*.

Mr. Owen commences by observing that the scientific world possesses ample experience of the truth and tact with which the illustrious Cuvier formed his judgements of the affinities of an extinct animal from the inspection of a fossil fragment; and that it is only when so distinguished a comparative anatomist as M. de Blainville questions the determinations, that it becomes the duty of those who possess the means to investigate the nature of the doubts, and reassure the confidence of geologists in their great guide.

When Cuvier first hastily examined at Oxford, in 1818, one of the jaws described in this paper, and in the possession of Dr. Buckland, he decided that it was allied to the Didelphys (me semblèrent de quelque Didelphe†); and when doubts were raised by M. Constant Prevost, in 1824‡, relative to the age of the Stonesfield slate, Cuvier, from an examination of a drawing made for the express purpose, was confirmed in his former determination; but he added, that the jaw differs from that of all known carnivorous Mammalia, in having ten molars in a series in the lower jaw: ("il [the drawing] me confirme dans l'idée que la première inspection m'en avoit donnée. C'est celle d'un petit carnassier dont les mâchelières ressemblent beaucoup à celles des sarigues; mais il y a dix de ces dents en série, nombre que ne montre aucun carnassier connu." Oss. Foss. 111. 349. note.) It is to be regretted that the particular data, with the exception of the number of the teeth, on which Cuvier based his opinion, were not detailed; but he must have been well aware that the grounds of his belief would be obvious, on an inspection of the fossil, to every competent anatomist: it is also to be regretted that he did not assign to the fossil a generic name, and thereby have prevented much of the reasoning founded on the supposition that he considered it to have belonged to a true Didelphys.

Mr. Owen then proceeded to describe the structure of the jaw; and he stated that having had in his possession two specimens of the *Thylacotherium Prevostii* belonging to Dr. Buckland, he has no hesitation in declaring that their condition is such as to enable any anatomist conversant with the established generalizations in comparative osteology, to pronounce therefrom not only the class but the more restricted group of animals to which they have belonged. The specimens plainly reveal, first, a convex articular condyle; secondly, a well-defined impression of what was once a broad, thin, high, and slightly recurved, triangular, coronoid process, rising immediately anterior to the condyle, having its basis extended over the whole of the interspace between the condyle and the commencement of the molar series, and having a vertical diameter equal to that of the horizontal ramus of the jaw itself: this impression also exhibits traces

* This has since been read, and an abstract of it will appear in our next number.—Edit.

† Ossemens Foss., tome iii. p. 349.

‡ Annales des Sciences Nat., Avril, 1825; also the papers of Mr. Broderip and Dr. Fitton in the Zoological Journal, 1828, vol. iii., p. 409.

of the ridge leading forwards from the condyle and the depression above it, which characterizes the coronoid process of the zoophagous marsupials; thirdly, the angle of the jaw is continued to the same extent below the condyle as the coronoid process reaches above it, and its apex is continued backwards in the form of a process; fourthly, the parts above described form one continuous portion with the horizontal ramus of the jaw, neither the articular condyle nor the coronoid being distinct pieces as in reptiles. These are the characters, Mr. Owen believes, on which Cuvier formed his opinion of the nature of the fossil; and they have arrested the attention of M. Valenciennes in his endeavours to dissipate the doubts of M. de Blainville*.

From the examination of a cast, the latter, however, has been induced to infer that there is no trace of a convex condyle, but in place thereof an articular fissure, somewhat as in the jaws of fishes; that the teeth, instead of being imbedded in sockets, have their fangs confluent with or anchylosed to the substance of the jaws, and that the jaw itself presents evident traces of the composite structure.

In answer to the first of these positions, Mr. Owen states that the portion of the true condyle which remains in both the specimens of Thylacotherium examined by Cuvier and M. Valenciennes, clearly shows that the condyle was convex, and not concave. It is situated a little above the level of the grinding surface of the teeth, and projects beyond the vertical line, dropped from the extremity of the coronoid process, but not to the same extent as in the true Didelphys. In the specimen examined by M. Valenciennes, the condyle corresponds in position with that of the jaw of the Dasyurus rather than the Didelphys; it is convex, as in mammiferous animals, and not concave as in oviparous. The entire convex condyle exists in the specimen belonging to the other genus, Phascolotherium, now in the British Museum, but formerly in the cabinet of Mr. Broderip. Mr. Owen is of opinion that the entering angle or notch, either above or below the true articular condyle, has been mistaken for "une sorte d'échancrure articulaire, un peu comme dans les poissons."

The specimen of the half-jaw of the Thylacothere examined by M. Valenciennes, like that [the drawing of?] which was transmitted to Cuvier, presents the inner surface to the observer, and exhibits both the orifice of the dental canal and the symphysis in a perfect state. The foramen in the fossil is situated relatively more forward than in the recent Opossum and Dasyure, or in the Placental Insectivora, but has the same place as in the marsupial genus *Hypsiprymnus*. The symphysis is long and narrow, and is continued forward in the same line with the gently convex inferior margin of the jaw, which thus tapers gradually to a pointed anterior extremity, precisely as in the jaws of the Marsupial Insectivora. In the relative length of the symphysis, its form and position, the jaw of the Thylacotherium precisely corresponds with that of the Didelphys.

* Comptes Rendus, 1838; Second Semestre, No. 11, Sept. 10, p. 527 *et seq.*

In addition, however, to these proofs of the mammiferous nature of the Stonesfield remains, and in part of their having belonged to Marsupialia, Mr. Owen stated that the jaws exhibit a character hitherto unnoticed by the able anatomists who have written respecting them, but which, if co-existent with a convex condyle, would serve to prove the marsupial nature of a fossil, though all the teeth were wanting.

In recent marsupials the angle of the jaw is elongated and bent inwards in the form of a process, varying in shape and development in different genera. In looking, therefore, directly upon the inferior margin of the marsupial jaw, we see in place of the edge of a vertical plate of bone, a more or less flattened triangular surface or plate of bone extended between the external ridge and the internal process or inflected angle. In the Opossum this process is triangular and trihedral, and directed inwards with the point slightly curved upwards and extended backwards, in which direction it is more produced in the small than in the large species of Didelphys.

Now, if the process from the angle of the jaw in the Stonesfield fossil had been simply continued backwards, it would have resembled the jaw of an ordinary placental carnivorous or insectivorous mammal; but in both specimens of Thylacotherium the half-jaws of which exhibit their inner or mesial surfaces, this process presents a fractured outline, evidently proving that when entire it must have been produced inwards or mesially, as in the Opossum.

Mr. Owen then described in great detail the structure of the teeth, and showed, in reply to M. de Blainville's second objection, that they are not confluent with the jaw, but are separated from it at their base by a layer of matter of a distinct colour from the teeth or the jaw, but evidently of the same nature as the matrix; and secondly, that the teeth cannot be considered as presenting an uniform compressed tricuspid structure, and being all of one kind, as M. de Blainville states, but must be divided into two series as regards their composition. Five if not six of the posterior teeth are quinque-cuspidate and are *molares veri*; some of the *molares spurii* are tricuspid and some bicuspid, as in the Opossums. An interesting result of this examination is the observation that the five cusps of the tuberculate molares are not arranged, as had been supposed, in the same line, but in two pairs placed transversely to the axis of the jaw, with the fifth cusp anterior, exactly as in the Didelphys, and totally different from the structure of the molares in any of the Phocæ, to which these very small Mammalia have been compared: and in reference to this comparison, Mr. Owen again calls attention to the value of the character of the process continued from the angle of the jaw, in the fossils, as strongly contradistinguishing them from the Phocidæ, in none of the species of which is the angle of the jaw so produced. The Thylacotherium differs from the genus Didelphys in the greater number of its molars, and from every ferine quadruped known at the time when Cuvier formed his opinion respecting the nature of the fossil. This difference in the number of the molar teeth, which Cuvier urged as evidence of the generic distinction of the Stonesfield mammiferous

fossils, has since been regarded as one of the proofs of their Saurian nature; but the exceptions by excess to the number seven, assigned by M. de Blainville to the molar teeth in each ramus of the lower jaw of the insectivorous Mammalia, are well established, and have been long known. The insectivorous Chrysochlore, in the order Feræ, has eight molars in each ramus of the lower jaw; the insectivorous Armadillos have not fewer; and in one subgenus (Priodon) there are more than twenty molar teeth on each side of the lower jaw. The dental formulæ of the carnivorous Cetacea, again, demonstrate the fallacy of the argument against the mammiferous character of the Thylacotherium founded upon the number of its molar teeth. From the occurrence of the above exceptions in recent placental Mammalia, the example of a like excess in the number of molar teeth in the marsupial fossil ought rather to have led to the expectation of the discovery of a similar case among existing marsupials, and such an addition to our zoological catalogues has, in fact, been recently made. In the Australian quadruped described by Mr. Waterhouse under the name of Myrmecobius* an approximation towards the dentition of the Thylacotherium is exemplified, not only in the number of the molar teeth, which is nine on each side of the lower jaw in the Myrmecobius, but also in their relative size, structure, and disposition. Lastly, with respect to the dentition, Mr. Owen says it must be obvious to all who inspect the fossil and compare it with the jaw of a small Didelphys, that contrary to the assertion of M. de Blainville, the teeth and their fangs are arranged with as much regularity in the one as in the other, and that no argument of the Saurian nature of the fossil can be founded on this part of its structure.

With respect to M. de Blainville's assertion that the jaw is compound, Mr. Owen stated, that the indication of this structure near the lower margin of the jaw of the Thylacotherium is not a true suture, but a vascular groove similar to that which characterizes the lower jaw of Didelphys, Opossum, and some of the large species of Sorex.

In a memoir to be brought forward on another occasion, Mr. Owen intends to describe the other genus found at Stonesfield, and for which, on account of its marsupial affinities, he proposes the name of *Phascolotherium*.†

A notice by R. W. Fox, Esq., was afterwards read, "On the Formation of Metallic Veins by Voltaic Agency."‡

In this communication Mr. Fox says, that he has succeeded not only in forming well-defined metalliferous veins in a crack in the middle of masses of clay by means of voltaic agency, but also in imparting to the clay a laminated or schistose structure; the veins and laminæ being perpendicular to the voltaic forces. In some instances

[* See Lond. & Edinb. Phil. Mag. vol. ix. p. 520; vol. xi. p. 200.—Edit.]

[† A list of the fossils associated with these marsupial remains in the Stonesfield slate, drawn up by Mr. De la Beche, will be found in Phil. Mag. and Annals, N. S. vol. vii. p. 257.—Edit.]

[‡ See Lond. and Edinb. Phil. Mag. vol. xi. p. 203.—Edit.]

only a pair of plates, or in preference copper pyrites and zinc, were employed to produce the voltaic action; but a constant battery consisting of several pairs of plates was much more effective. Among the veins thus produced in clay, Mr. Fox mentions oxide and carbonate of copper, carbonate of zinc, oxides of iron and tin. Veins of carbonate of zinc were formed, sufficiently firm to admit of being taken out in plates of the size of a shilling. Mr. Fox then describes a vein formed in pipeclay, by Mr. Jordan, by five pairs of cylinders, in three weeks. The clay divided an earthenware vessel into two cells, into one of which, containing the copper plate, a solution of sulphate of copper was put; and into the other, or zinc cell, a solution of common salt. Well-defined veins were thus produced of carbonate and oxide of copper, and carbonate of zinc, parallel to the laminæ into which the clay divided; as well as another of carbonate and oxide of copper at right angles to them. On dividing the mass of clay in the direction of the principal horizontal vein, the carbonate of zinc was found on the negative side, or towards the copper plate; and the carbonate of copper nearest the zinc plate: and as the former must have been derived from the zinc plate, it is curious to observe such a complete transposition of the respective metals.

Mr. Fox is of opinion that these results have a strong bearing on the numerous mineral veins and beds which are found conformable to the direction of the laminæ of the containing rocks, as well as on those veins which traverse the laminæ of the conformable veins.

An extract was afterwards read from a letter addressed by Captain Alexander to the Secretary, explanatory of casts of portions of Mastodon teeth from the crag, and on the occurrence of a particular bed containing Echini in the coralline crag * at Sudbourne.

The larger cast was taken from a Mastodon tooth found on the shore at Sizewell Gap, about seven miles from Southwold. When the original came into Captain Alexander's possession, crag adhered to it in considerable quantity; and he has no doubt that it had been washed from Easton, about $1\frac{1}{2}$ mile north of Southwold. The weight of the tooth is 2 lbs. $5\frac{1}{2}$ oz., its length is about 6 inches, and its breadth $3\frac{1}{2}$ inches; and although it had been washed eight miles, only three of the crowns had been injured. The other cast is from a fragment of a young tooth found by the author in the crag at Bramerton.

Capt. Alexander found also the canine tooth of a large carnivorous animal in the crag at Easton. At Bramerton he obtained also five crabs, three of which were almost perfect. At Sudbourne, near Orford, in a bed of very fine coralline crag, he found several beautiful Echini; and in a thin, argillaceous layer in the centre of the same bed, the greater part of the vertebral column of a fish, the remains of crabs, and the ear bone of a whale, which had apparently been water-worn before it was enclosed in the crag. To this stratum Captain Alexander calls particular attention, as he believes it would be found to be rich in organic remains, if it were properly examined.

Dec. 5th 1838.—A paper was first read, entitled " A few brief

[* See Lond. and Edinb. Phil. Mag., vol. vii. p. 83.—Edit.]

Remarks on the Trap Rocks of Fife," by the Rev. John Fleming, D.D., and communicated by Charles Lyell, Esq., V.P.G.S.

The trap rocks of Fifeshire are referred by Dr. Fleming to three distinct epochs of volcanic action; and he says that the products of each epoch are not more decidedly characterized by dissimilarity in their relationship to the associated sedimentary rocks than by differences in their composition.

The traps of the first epoch occupy the northern portion of the county from Stratheden to the estuary of the Tay, constituting the eastern extremity of the Ochils. They appear to be coeval with the grey sandstone (Arbroath pavement), and to rest upon, as well as to be variously associated with the old red sandstone, and to be covered by the yellow sandstone which supports the mountain limestone. Viewed on a great scale, they consist of amygdaloids containing irregular masses of porphyry, clay-stone, clink-stone, compact felspar, green-stone, and trap tuff: they also contain thin layers of slate-clay and grey sandstone. The whole of the igneous rocks are decidedly stratified; and though the beds are thick and variously bent, they have, in general, the same dip as the superior and inferior sedimentary formations. The materials of which they are composed, Dr. Fleming conceives were spread out under water, partly as lava and partly as ashes; and that several of the peculiarities of rocky structure have been produced by corpuscular action.

Two vertical greenstone veins traverse this group in an easterly direction. One of them may be traced along the north side of the Ochils from the neighbourhood of Newburgh by Norman's Law to Luthrie, a distance of nearly six miles: the other, observable at Alva and Dollard, on the south side of the Ochils, may be traced nearly forty miles by Monymeal to Hilton Bridge, north of Cupar. Several cross veins of greenstone and felspar likewise occur.

The trap rocks of the second epoch form the southern margin of Stratheden, and may be considered as constituting a ridge parallel with the Ochils, from near St. Andrews to Stirling; but several branches or patches of the same age have been observed in the counties on the south of the Forth. These traps consist almost exclusively of greenstone, which in a few instances is earthy and amygdaloidal. They cover, in many places, the lower beds of the coal-measures; on the East Lomond they are intermixed with the mountain limestone; and at Wemyss Hall Hill, south of Cupar, they overlap the limestone, and are in contact with the yellow sandstone.

These two groups of trap rocks, the author is of opinion, were produced while the associated strata of old red sandstone and coal-measures were horizontal; and that they have undergone, equally with the sedimentary formations, the movements which gave the strata of the Ochils and the ridge south of Stratheden the southerly dip. He is also of opinion, that the greenstone of the second group may have furnished materials for the great veins, which traverse the older one.

The traps of the third epoch occur chiefly along the shores of the Forth, and in the higher coal-measures. They consist of basalt with

olivine, amygdaloid, greenstone, wacke, and trap tuff; and they frequently contain fragments of limestone, flinty slate, slate-clay, bituminous shale, sandstone, and coal. They appear to have been produced while the associated sedimentary strata were horizontal, and to have undergone with them the same disturbing movements*.

An account of Footsteps of the Chirotherium†, and other unknown animals lately discovered in the quarries of Storeton Hill, in the peninsula of Wirrall, between the Mersey and the Dee, communicated by the Natural History Society of Liverpool, and illustrated with drawings by John Cunningham, Esq., was then read.

In the early part of last June, there were discovered in the Storeton quarries, on the under surface of several large slabs of sandstone, highly relieved casts of what the workmen believed to have been human hands; and the circumstance having been made known to the Natural History Society of Liverpool, a committee was appointed, who drew up the report communicated to this Society.

The peninsula of Wirrall consists of new red sandstone; and towards the northern extremity, the formation may be separated into three principal divisions. The lowest is composed of beds, slightly inclined towards the east, of red or variegated sandstone, occasionally abounding with pebbles partly derived from the coal-measures; and in the bottom strata either angular or little water-worn. Seams of marl are very rare in this division, the argillaceous matter being confined to nodules or concretions of clay of the same colour as the sandstone.

The middle division consists of white or yellow sandstone, in some places argillaceous, and frequently containing round concretions of clay, and pebbles. The strata are separated by seams of white or mottled clay, occasionally almost imperceptible, but sometimes several inches thick.

The uppermost division is formed of red or variegated sandstone, inclosing also nodules of clay and pebbles of quartz; and it abounds with strata of red marl.

The Storeton quarries are situated in the middle division; and the casts which have hitherto been noticed, occurred on the under surface of three beds of sandstone, about two feet thick each. The strata incline 8° to the north-east, but they are traversed by several faults, which range in the strike of the beds. The authors of the re-

* For further particulars, see Mackenzie on the Ochils, Mem. Wern. Soc., vol. ii. p. 1; Fleming on Scales in the Old Red Sandstone of Fifeshire, Edinb. Journ. Nat. and Geograph. Science, Feb. 1831; and on the Mineralogy of the Neighbourhood of St. Andrews, Mem. Wern. Soc., vol. ii. p. 145; also Neill's Daubuisson, p. 215.

† This name was first applied provisionally by Professor Kaup, to similar casts discovered, towards the end of 1834, in the sandstone quarries at Hesseberg, near Hildburghausen. See Dr. F. R. L. Sickler's Letter to Blumenbach, 1834; also, Die Plastik der Urwelt im Werrathale bei Hildburghausen, with plates by C. Kepler, and an introduction by Dr. Sickler, 1st part, 1836; and Dr. Buckland's Bridgewater Treatise, 1836.

port are of opinion, that each of the thin seams of clay in which the sandstone casts were moulded, formed successively a dry surface, over which the Chirotherium and other animals walked, leaving impressions of their footsteps; and that each layer was submerged by a depression of the surface. The lowest seam of clay was so thin, that the marks penetrated into the subjacent sandstone. The following account is then given of a hind foot and a fore foot, selected from slabs in the Museum of the Royal Institution, Liverpool.

Hind Foot, consisting of five digits; one of which, from its resemblance to a human thumb, has been generally distinguished by that designation.

	Inches.
Total length from the root of the thumb to the point of the second toe	9
Extreme breadth from the point of the thumb to the point of the fourth toe	6
Breadth across the toes	5
Breadth across the palm	3
Length of the curved line extending from the root of the thumb to its point	$6\frac{1}{2}$
Breadth of the ball of the thumb	$1\frac{1}{2}$
Relief of the ball of the thumb from the surface of the slab	$\frac{1}{2}$
Length of the first toe from the root to the point	$5\frac{1}{4}$
Length of the second ditto	$5\frac{1}{2}$
Length of the third ditto	4
Length of the fourth ditto	$2\frac{1}{2}$
Average breadth of the first three toes	1
Average breadth of the fourth toe rather less than	1
Relief of the second toe, which presents the greatest prominence	$\frac{6}{10}$

One hind foot has been observed which measured 12 inches in its greatest length.

Judging from the appearance of the casts, the sole of the foot must have been amply supplied with muscles, the casts of the ball of the thumb and the phalanges of the fingers being prominent. The digit, which has been called a thumb, is of a tapering shape, and is bent backwards near the extremity, where it ends in a point. It is extremely smooth, and there is no satisfactory evidence of either a nail or a claw. The toes are thick and strong, and had probably three phalanges each, and at the terminations are traces of stout, conical nails or claws. The sole of the foot is supposed to have been covered by a slightly rugose skin, the folds of which are stated to be distinctly visible in the casts of the toes.

Fore Foot. Perfect impressions of the fore feet are extremely rare, owing either to the animal having used those feet lightly, or to the impressions having been obliterated by the tread of the hind feet. The best preserved cast exhibits a thumb and three toes, being deficient of the fourth. The dimensions, which are generally half those of the hind foot, are as follows;

	Inches.
Length from the root of the thumb to the point of the second toe	$4\frac{1}{2}$
Total breadth not ascertained in consequence of the absence of the fourth toe.	
Breadth of the palm	$1\frac{3}{4}$
Length of the thumb	$2\frac{1}{2}$
Breadth of the ball of the thumb	1
Length of the first toe	2
Length of the second toe	$2\frac{1}{4}$
Length of the third toe	$2\frac{1}{4}$
Greatest breadth of the toes	$\frac{3}{4}$

The thumb is slightly bent back, and pointed, and the toes were armed with nails.

Traces of one animal have been observed in a continuous line on a slab ten yards long. The length of the step varies a little, but in general, the distance between the point of the second toe of one hind foot and the point of the same toe in the hind foot immediately in advance, is between 21 and 22 inches. Each fore foot is placed directly in front of the hind, and the thumbs of both extremities are always towards the medial line of the walk of the animal. Some further observations are given by the authors with respect to the progression of the animal, on the supposition that the digit conjectured to be a thumb, was really the first. Conceiving such to be the case, they state, that the animal must have crossed its feet three inches in walking, for the right fore and hind feet are placed $1\frac{1}{2}$ inch on the *left* side of the medial line, and the left fore and hind feet $1\frac{1}{2}$ inch on the right side of the same line.

The casts of the Chirotherium, although the most remarkable, are by no means the most numerous, which exist on the Storeton sandstones. Many large slabs are crowded with casts in rilievo, some of which are supposed to have been derived from the feet of saurian reptiles, and others from those of tortoises. Occasionally the webs between the toes can be distinctly traced. "It is impossible," say the authors of the report, "to look at these slabs and not conclude, that the clay beds on which they rested, must have been traversed by multitudes of animals, and in every variety of direction."

A note by Mr. James Yates was then read, giving a brief account of sketches of four differently characterized footsteps, traced from casts procured at Storeton, each of which is distinct both from the casts of the Chirotherium and the web-footed animal mentioned in the preceding report.

A paper was afterwards read "On two Casts in Sandstone of the impressions of the Hind Foot of a gigantic Chirotherium, from the New Red Sandstone of Cheshire," by Sir Philip Grey Egerton, Bart., M.P., F.G.S.

These specimens first came under the notice of Colonel Egerton about 1824, and they were placed in the author's cabinet in 1836; but it was not until the recent discovery of the Chirotherium at

Storeton, that their true nature was suspected. The exact locality, at which the specimens were discovered, is not known; but it is probable, that they were obtained from the neighbourhood of Colonel Egerton's residence, near Tarporley, and from one of the beds of sandstone, which alternate with the red and green marls in the upper part of the new red system in that part of Cheshire.

The casts, which consist of a rather soft and coarse sandstone, were evidently formed in the impressions of two hind feet; and though they have suffered from exposure to the weather for twelve years, yet they are sufficiently perfect to have enabled Sir Philip Egerton to take the measurements of the different parts, and draw up the accompanying comparative table. It is necessary to state, that though he preserves the use of the term thumb for the convenience of comparison with previous descriptions, yet he is of opinion that the marginal digit which has been so designated, is not the representative of the fifth, but of the first toe.

Direction of the Measurements.	Hessberg Chirotherium.			Storeton Chirotherium.			Large Chirotherium from near Tarporley.	
Length from the heel to the point of the 2nd toe	7	8	..	8	7	..	15	0
Length from the heel to the point of the thumb	3	4	..	4	3	..	8	0
Length from the heel to the angle between the 1st and 2nd toes	4	8	..	5	6	..	10	0
——— 2nd and 3rd toes	4	4	..	5	8	..	11	0
——— 3rd and 4th toes	4	0	..	5	3	..	11	0
Greatest breadth across the insertions of the toes	5	0	..	4	2	..	8	5
Breadth from the point of the thumb to 4th toe	5	5	..	5	0	..	9	0
Breadth from the thumb to point of 4th toe	6	3	..	6	0	..	10	6
Breadth across the sole below the thumb ..	3	6	..	3	0	..	6	0
Breadth from 1st toe-point to 4th toe-point	4	6	..	4	6	..	9	0

From these measurements it appears, that considerable differences exist in the three specimens of Chirotherium. Upon comparing the footstep from Hessberg with that from Storeton, it will be found, that the former is thicker and more clumsy than the latter; that the sole is shorter and broader, and the toes wider and longer. The most important discrepancy, however, is in the position of the thumb, which is placed much nearer the heel in the Hessberg specimens than in those from Storeton. The cast from near Tarporley resembles the latter more than the former; it nevertheless differs considerably in the proportion of the breadth to the length of the sole, which is greater; and in the proportions of the length of the toes to the length of the sole, which is less than in the Storeton specimens. It is also distinguished by the greater divergence of the toes from each other. From these differences and the gigantic size of the Tarporley specimen, the author conceives that the animal which made the impression was a distinct species; and he proposes for it, in compliance with the adage *ex pede Herculem*, the name of *Chirotherium Herculis*.

CAMBRIDGE PHILOSOPHICAL SOCIETY.

Nov. 5.—The anniversary meeting of this society was held on Monday evening last, the Rev. Dr. Graham, the President, being in the chair. The Treasurer's report was read, and the audit confirmed; and the following officers were elected for the ensuing year:

Dr. Graham, President, (re-elected.)

Mr. Hopkins (re-elected), Dr. Clarke, Prof. Cumming, Vice-Presidents.

Prof. Peacock, Treasurer.

Prof. Henslow, Prof. Whewell, Prof. Willis, Secretaries.

Old Council.	New Council.
Rev. J. Power,	Dr. Bond,
Prof. Miller,	Dr. Paget,
Prof. Challis,	Mr. Stokes,
Rev. J. W. Barnes,	Mr. Earnshaw,
Prof. Sedgwick,	Mr. Garnons,

Nov. 12.—Mr. Hopkins, one of the Vice-Presidents, in the chair. Professor Whewell made a communication "respecting certain kinds of Architecture."

Nov. 26.—Dr. Graham, the President, in the chair. A paper was read by Mr. D. T. Gregory, of Trinity College, "On the Logarithms of Negative Quantities": and a communication was made by Professor Henslow on the formation of mineral veins, as illustrated by a specimen of "brown clay" from Suffolk.

Dec. 10.—Dr. Graham, the President, in the chair. A communication, by Mr. Holditch, of Caius College, was laid before the Society, "On rolling curves." Professor Willis, who stated the results at which Mr. Holditch had arrived, illustrated the subject by models of revolving wheels of various forms, working in each other by rolling contact. These forms may consist of one, two, three, four, or more *lobes*: the form which consists of one lobe, may be an ellipse turning about its focus. A note on the Flora of Madeira, by Mr. Lowe, in addition to his Memoir, published in the last Part of the Society, was read; also the beginning of a paper by Mr. Rothman, "On the climate of Italy." Prof. Henslow also gave an account of the structure of wasps' nests.

XXVI. *Intelligence and Miscellaneous Articles.*

EQUIVALENT OF CARBON AND COMPOSITION OF NAPHTHALIN.

MM. Theard, Robiquet, and Dumas, in reporting upon the memoir of MM. Pelletier and Walter, having occasion to allude to the composition of naphthalin, state that one of them some years since analysed a very pure specimen of this substance, and found that 0·400 of it yielded 1·370 of carbonic acid and 0·222 of water; this representing

Carbon	94·76
Hydrogen	6·16
	100·92

These numbers, it is observed, though very nearly approaching those which give the formula $C^{40} H^{16}$, generally admitted, show an unusual excess of carbon; this circumstance was notified to M. Laurent, who was long occupied with naphthalin in theoretic considerations, and it is to be regretted that it did not receive his attention.

The recent analysis of naphthalin by Liebig also exhibits the same excess of carbon.

Carbon	94·3	94·2	94·6
Hydrogen	6·2	6·1	6·1
	100·5	100·3	100·7

The analysis of naphthalin is then decidedly different from that of the formula attributed to it. M. Dumas therefore considered it necessary to undertake some experiments on the naphthalin of resin, in order to compare it with that of coal.

Five experiments upon the naphthalin from the resin oil gave:

	I.	II.	III.	IV.	V.
Carbon	94·2	94·2	94·27	94·9	94·9
Hydrogen	6·3	6·3	6·26	6·2	6·1
	100·5	100·5	100·53	100·1	100·

M. Dumas then again analysed coal naphtha, and the results were

	I.	II.	III.
Carbon	94·55	94·2	94·55
Hydrogen	6·50	6·3	6·20
	101·05	100·5	100·75

In some of these analyses the quantity of carbon only was determined, which introduced errors into the hydrogen, that are usually avoided. As the hydrogen was never less than 6·2, it may be questioned whether the formula for naphthlalin ought not to be replaced by that by Liebig, which is,

C^{40}	1528·7	94·23
H^{15}	93·6	5·77
	1622·3	100·

On the other hand, it may be readily perceived, that a slight error in the atomic weight of carbon would be sufficient to explain the discrepancies between calculation and direct analysis; an example will immediately show this: 0·387 of naphthalin gave 1·318 of carbonic acid and 0·220 of water, which according to the atomic weight attributed to carbon, would give 94·2 per cent. of carbon in naphthalin.

But on the supposition that the atomic weight of carbon should be reduced to 38 instead of 38·6, [*i. e.* 6·08 hydrogen = 1.] this analysis would be represented as follows:

Carbon	93·8
Hydrogen	6·2
	100·

And if reduced to 100 parts according to the formula C^{40} H^{16}, according to this new atomic weight, we should have

C^{40}	1520·0	93·8
H^{16}	100·0	6·2
	1620·	100·

M. Dumas observes, that according to this hypothesis, the old formula for napththalin will remain correct, the atomic weight of carbon, inferred from the density of carbonic acid and that of oxygen gas, would however be incorrect.

It will be remembered by chemists, that some years since Berzelius represented the atomic weight of carbon by 75·33, which, from the results obtained in the analyses of organic substances, he raised to 76·52. More lately a fresh modification has brought it to 76·44, which is the number adopted by many chemists. It is impossible, observes M. Dumas, according to the analysis of naphthalin, that this last atomic weight can be correct, unless it be supposed that an error which much exceeds all probability can be attributed to hydrogen, hence it would be equal to about one-sixth of its weight; added to this, every thing indicates that there is no error in the atomic weight of hydrogen. It follows, therefore, that the atomic weight of carbon must be inaccurate; for 100 of naphthalin always give 6·2 of hydrogen, and 94·9, or at least 94·2 of carbon, which makes an excess of $\frac{4}{1000}$, and even of $\frac{11}{1000}$.

From these results will be seen the necessity of reducing the atomic weight of carbon to 76·0, or even 75·9, which last appears to be the most probable.—*Journal de Pharmacie,* vol. xxiv. p. 393.

[It will be observed that hydrogen $=1$, the atomic weight of carbon will be 6·07, which comes very near to the number hypothetically adopted by Dr. Prout, and admitted by Dr. Thomson, Dr. Henry, Mr. Brande, and most English chemists.—R. P.]

COMPOSITION OF WAX.

It having been supposed that wax contained margaric acid and peculiar compounds which were termed *cerain* and *cerin,* M. Hess has undertaken a fresh examination of this substance; he commenced by treating yellow wax with cold æther, which decolorises it in great part, and divides it into very delicate small scales; these were put in a filter and suffered to drain, and the coloured solution having run through, a fresh quantity of æther was added. The wax which remained undissolved by this second operation was separated, and twice melted with water; it was then white, hard, brittle, and melted at about 150° Fahr. Two analyses gave,

Hydrogen. . .	13·21	13·22
Carbon.	80·79	80·84
Oxygen. . . .	6·00	5·94
	100·	100·

The portion of wax dissolved by the first quantity of æther was separated from it by distillation with water. This, the more soluble

portion of the wax, was yellow, had a strong smell of fresh wax, which it resembled in every respect, and melted at nearly the same temperature. It was treated with a very small quantity of æther, to remove a portion of its colouring matter, and afterwards fused and analysed. It yielded,

Hydrogen. . .	13·16	
Carbon.	80·57	
Oxygen. . . .	6·27	100·

M. Hess infers that from the similarity of these results that the first portion dissolved by the æther is identical with the second portion, and consequently that wax is a uniform substance containing neither margaric acid, cerin, nor cerain.

It may be supposed that this conclusion is applicable only to Russian wax; but if we consider that M. de Saussure obtained a similar result with bees-wax; that M. Boussaingault obtained from the wax of the *Ceroxylon andicola*, after separating the resin,

Hydrogen. . .	13·1	13·3
Carbon.	81·2	81·6
Oxygen. . . .	5·7	5·1
	100·0	100·

no doubt can exist as to the identity of wax obtained in different places.

M. Oppermann has analysed two other kinds of wax; Japan wax yielded,

Hydrogen. . .	12·07	
Carbon.	70·97	
Oxygen. . . .	16·96	100·

If it be admitted that the amount of hydrogen in this analysis is incorrect to the same extent as that in bees-wax, which is a very likely circumstance, arising from the errors of manipulation, the hydrogen and the carbon will be found to be in the same proportions in both; for $C : H^2 = 70{\cdot}9 : 11{\cdot}57$. In this wax, 100 parts of carburetted hydrogen are combined with 20·4 of oxygen, which is rather more than $3\frac{1}{2}$ times that in bees-wax.

To Brazilian wax M. Oppermann assigns as the composition,

Hydrogen. . .	12·03	
Carbon.	72·77	
Oxygen. . . .	15·80	100·

which agrees exactly with this mode of regarding the subjects, since $C : H^2 = 72{\cdot}87 : 11{\cdot}896$, which differs from the number stated within the limits of faults of manipulation; 100 parts of carburet of hydrogen are found combined with 17·7 of oxygen, which is almost identical with the result obtained by M. Hess by treatment with nitric acid.

All the characters of this substance which M. Oppermann assigns to this substance are confirmed by M. Hess; it appears therefore to him very probable that in these cases the same radical occurs in two different degrees of oxidizement, and M. Hess remarks that it would therefore be extremely interesting to acquire positive information

respecting the vegetables which produce the Japanese and Brazilian wax, and as to the mode in which they are extracted; it may perhaps be found whether this oxidizement depends on the nature of the plant, or merely on that of the organ which produces the wax, or on the time of collecting it.—*L'Institut*, No. 259.

AMILEN. OIL OF POTATOES.

M. Auguste Cahours on considering the numbers derived from the composition and density of the vapour of this oil, was induced to believe that it is a true alcohol, isomorphous [isomeric ?] with common alcohol, pyroxilic spirit and ethal: he therefore undertook a series of experiments to verify these hypotheses; and he conceives that the oil of potatoes, and the compounds derived from it, contain a radical C^{40} H^{40}*, which may be readily separated, and to which he gives the name of *Amilen.*

When oil of potatoes is repeatedly distilled with anhydrous phosphoric acid, a colourless, oily, very limpid liquid is obtained, which boils at about 320° Fahr.; this is shown by analysis to be a true carburetted hydrogen, of the same composition as olefiant gas and methylen, and differing from them only in the state of condensation of its elements. The numbers deduced from analysis, and the determination of the density of its vapour, give C^{40} H^{40} as its natural formula, which represent four volumes of vapour. It therefore presents an anomaly which does not attend either olefiant gas or methylen; and whilst in the alcohols which correspond to these carburets, four volumes of vapour are formed of four volumes of vapour of carburetted hydrogen, and four volumes of vapour of water, this new alcohol contains only two volumes of vapour of carburetted hydrogen and four volumes of the vapour of water.—*L'Institut*, No. 260.

ACTION OF CHLORIDE OF ZINC UPON ALCOHOL.

M. Dumas has read a report respecting the memoir of M. Masson on the above subject. The author dissolves chloride of zinc in alcohol, and subjects the mixture to distillation, receiving the products in different portions. When the liquor boils, it yields at first alcohol; but when the point of ebullition is a little raised, and reaches about 284°, it yields sulphuric æther. Thus the chloride of zinc acts upon alcohol, exactly like concentrated sulphuric acid, and these two substances occasion the production of æther precisely at the same temperature. On continuing the process, an oil appears which perfectly recalls the characters of that known by the name of *sweet oil of wine*. It forms at about 320° Fahr., that is to say, very nearly under the same circumstances which occasion its formation when prepared from sulphuric acid and alcohol. It is further observable that the æther formed is accompanied with a certain quantity of water, and the oil which distils with a considerable quantity. These phænomena also attend the reaction of sulphuric acid upon alcohol; and M. Masson has further ascertained that no hydrochloric acid is

* The original symbols are preserved unaltered.

formed. It is then proved, by these experiments, that chloride of zinc acts like sulphuric acid itself; in fact the analogy between these substances is so perfect, that M. Masson has been recommended to examine whether some product analogous to sulphovinic acid is not also formed.

The oil obtained by M. Masson was found by him to contain two different products, which were separated by distillation. One of them, and the most volatile, is the most hydroguretted liquid carburet known; it contains more hydrogen than olefiant gas, and is represented by $C^8 H^9$; it boils at about 85° to 105° Fahr. The other, which is less volatile, contains less hydrogen than olefiant gas, and is represented by $C^8 H^7$, and boils at about 572° Fahr.

These results, combined with those by which M. Regnault has shown that the sweet oil of wine absorbs oxygen, perfectly explain why some chemists have obtained in their analysis more carbon than olefiant gas contains; and why others, on the contrary, have obtained the same composition as olefiant gas itself.

The facts thus stated would have appeared sufficient to show that the labours of M. Masson were of such a nature as to close the discussions relating to the sweet oil of wine. But a German chemist, M. Marchand, has lately published some analyses of heavy oil of wine, light oil of wine, and the crystals which it yields. His results agree perfectly with those of M. Serullas, and consequently differ from those of M. Masson.

On considering the circumstance that some chemists have operated on the oil obtained by sulphuric acid and alcohol, others upon the oil of the sulphovinates, and that M. Masson procured his from alcohol and chloride of zinc, it may be imagined that these oils differ from each other, especially as M. Masson has never been able to extract from his oil the crystals obtained by M. Serullas and M. Marchand from theirs, and on the contrary that he has obtained a very volatile product unknown to the chemists who preceded him: M. Marchand has however procured during the distillation of the sulphovinate a very volatile product which he has not yet analysed. —*L'Institut*, No. 261.

ACTION OF SPONGY PLATINA.

M. Kuhlmann has described several new reactions determined by spongy platina. Among which are the following:

1st. Ammonia mixed with air on passing at a temperature of about 572° Fahr. over spongy platina is decomposed, and the azote which it contains is completely converted into nitric acid by combining with the oxygen of the air.

2nd. Cyanogen and air, under similar circumstances, occasion the formation of nitric acid and carbonic acid.

3rd. Ammonia, when combined so as to form a salt, acts in the same way as free ammonia.

4th. Free azote cannot in any case be combined with free oxygen, but all the compounds of azote, under the influence of spongy platina, yield nitric acid.

5th. Nitrous and nitric oxides, hyponitric and nitric acids mixed with a sufficient quantity of hydrogen, are converted into ammonia by their contact with spongy platina, and frequently without even the assistance of heat. The action is frequently so energetic that violent explosion ensues. All the azote of these oxides or acids passes to the state of ammonia, by combining with the hydrogen. An excess of nitric acid gives nitrate of ammonia.

6th. Cyanogen and hydrogen give hydrocyanate of ammonia.

7th. Olefiant gas and excess of nitric oxide, when hot and passed over spongy platina, produce carbonate and hydrocyanate of ammonia and water.

8th. With nitric oxide and excess of the vapour of alcohol, there are obtained under the same circumstances, the same compounds as above, and olefiant gas and a deposit of carbon.

9th. Free azote could not be combined with free hydrogen, but all the compounds of azote were converted into ammonia by hydrogen, either free or carburetted.

10th. In the last-mentioned reactions the presence of carbon in combination with azote or with hydrogen, occasions the formation of hydrocyanic acid.

11th. All the gaseous or vaporizable metalloids, without any exception, combine with hydrogen under the influence of spongy platina.

12th. The vapours of nitric acid mixed with hydrogen are totally converted into acetic æther, and water, at a moderate temperature.

M. Kuhlmann remarks that when precipitated platina (noir de platine) is substituted for spongy platina, the action is infinitely less energetic in the greater number of cases, which is the reverse of what might be expected. The precipitated platina has indeed no power in producing nitric acid, and it is very weak in producing ammonia, and it never becomes incandescent as happens with spongy platina; but in converting acetic acid into æther, the action of precipitated platina is on the contrary more quick, and produces it even at common temperatures.

It has been subsequently remarked that Berzelius has before stated that when nitric oxide is mixed with hydrogen gas, and the mixture exposed to partly calcined spongy platina, water and ammonia are gradually formed, on account of the union of the hydrogen with both the elements of the nitric oxide.—*Traité de Chemie,* ii. 43–44. *L'Institut,* No. 261—262.

ARCHITECTURAL SOCIETY.

Lectures on the Properties and Natural History of the Mineral Substances employed in the Arts of Architecture and Sculpture; by E. W. Brayley, Jun., F.L.S., F.G.S., Corr. Mem. Roy. Geol. Soc. of Cornwall, &c.

These lectures will be continued at the Apartments of the Architectural Society, 35, Lincoln's Inn Fields, on the following evenings, at 8 o'clock.

February 12*th,* 1839. On limestone and other substances affording materials for cements.

March 12*th.* On artificial substances employed as substitutes for stone.

April 9*th.* On those physical and chemical properties of building stones upon which their applications essentially depend.

FRENCH EXPEDITION OF DISCOVERY TO THE SOUTH POLAR SEAS.

This expedition, undertaken by the French Government under the command of M. d'Urville, has completely failed. The vessels, Astrolabe and Zélée, were not able to penetrate beyond the 64° south, being fully 10° short of the parallel reached by Weddel. They were stopped by a compact barrier of ice, and found the whole sea in the latitude we have mentioned completely frozen.—*Ann. Nat. Hist.*, No. XI.

CURIOUS HABIT OF EARTH-WORMS.

While staying at Whitley, near North Shields, Mr. Fryer pointed out to me that the worms (*Lumbrici*), which are abundant on the south side of his gravel walk, just under the shade of the tuft, where the walks are seldom used, gather together in a head all the loose stones within 6 or 8 inches of their hole, and heap them over its opening, sometimes to a considerable height. The holes when the stones are removed are large, and there are often also a few straws projecting from them. I do not recollect to have observed any similar habit in the worm in the neighbourhood of London; they are probably a different species.—J. E. GRAY.—*Ann. Nat. Hist.*

METEOROLOGICAL OBSERVATIONS FOR DECEMBER 1838.

Chiswick.—Dec. 1. Clear: rain. 2. Fine: heavy showers, with strong wind and thunder and lightning at night. 3, 4. Fine. 5. Drizzly. 6. Fine. 7. Rain. 8. Clear: frosty. 9. Frosty and foggy. 10. Frosty: fine. 11—13. Hazy. 14, 15. Foggy in the mornings: fine. 16—19. Hazy and cold. 20. Frosty: fine. 21. Hazy. 22—24. Rain. 25. Very clear. 26. Frosty: heavy rain at night. 27. Clear. 28. Frosty. 29. Overcast: rain. 30. Rain. 31. Clear and fine.

Boston.—Dec. 1. Fine: rain P.M. 2. Fine: rain early A.M. 3. Fine. 4. Fine: rain early A.M. 5. Cloudy. 6. Fine. 7. Cloudy: rain early A.M. 8—10. Fine. 11—13. Cloudy. 14—16. Fine. 17, 18. Cloudy. 19. Cloudy: rain P.M. 20, 21. Cloudy. 22—24. Rain. 25. Fine: snow early A.M.: snow P.M. 26. Fine: rain P.M. 27, 28. Fine. 29, 30. Cloudy. 31. Fine.

Applegarth Manse, Dumfries-shire.—Dec. 1. Dull and cloudy: wet P.M. 2. Frequent showers. 3. Dull and cloudy: temperate. 4. Cleared up: very mild. 5. Clear sunshine. 6. Dull and cloudy: shower P.M. 7. Generally clear: shower P.M. 8. Fine day: frosty P.M. 9. Dry, but cloudy. 10. Cloudy and moist. 11. Clear and frosty. 12. Thick fog A.M.: cleared: moist. 13. Cloudy and raw. 14. Clear and sharp. 15. Dull and threatening. 16. Fog: cleared up P.M. 17. Dull and cloudy. 18. Cloudy A.M., but cleared up. 19. Cloudy and threatening: rain. 20. Raw after rain preceding evening. 21. Clear and cold: wet P.M. 22. Dry and cold, but threatening rain. 23. Dry: still looking dull. 24. Dry A.M.: moist P.M. 25. Dry and clear: freezing P.M. 26. Wet and stormy till evening. 27. Frosty: ground very slippery. 28. Frost A.M.: thawed P.M. 29. Clear, but still soft. 30. Wet and stormy all day. 31. Showery and unsettled.

Meteorological Observations made at the Apartments of the Royal Society by the Assistant Secretary, Mr. Roberton; *by* Mr. Thompson *at the Garden of the Horticultural Society at Chiswick, near London; by* Mr. Veall *at Boston, and by* Mr. Dunbar *at Applegarth Manse, Dumfries-shire.*

Days of Month. 1838. Dec.	Barometer.						Thermometer.								Wind.				Rain.				Dew-point.
	London: Roy. Soc. 9 a.m.	Chiswick.		Boston. 8½ a.m.	Dumfries-shire.		London: Roy. Soc.			Chiswick.		Boston. 8½ a.m.	Dumfries-shire.		London: Roy. Soc. 9 a.m.	Chiswick 1 p.m.	Bost.	Dumfries-shire.	London: Roy. Soc. 9 a.m.	Chiswick.	Boston.	Dumfries-shire.	Lond.: Roy. Soc. 9 a.m.
		Max.	Min.		9 a.m.	9 p.m.	Fahr. 9 a.m.	Self-register. Max.	Min.	Max.	Min.		9 a.m.	9 p.m.									
○ 1.	29·552	29·557	29·345	29·02	29·29	29·03	46·7	52·0	45·2	56	48	47	42½	48½	S.	SW.	calm	SW.	...	·22	...	...	42
2.	29·464	29·493	29·432	28·90	29·05	29·05	52·7	53·2	46·2	50	44	51	47	46½	S.	SW.	calm	S.	·144	·51	·07	...	46
3.	29·464	29·476	29·457	28·90	29·12	29·15	46·7	55·0	46·0	53	39	45	45½	43	S.	SW.	calm	S.	·577	·02	...	...	44
4.	29·564	29·937	29·558	29·03	29·42	29·55	42·2	48·8	42·5	50	34	41·5	40½	39	SW.	W.	calm	SW.	...	...	·06	...	41
5.	29·834	30·131	29·871	29·33	29·85	30·10	40·9	48·6	38·9	48	35	43	42½	33	W.	NW.	calm	SW-N	...	·01	...	0·92	40
6.	30·256	30·304	30·276	29·76	30·12	30·09	41·8	47·2	40·8	47	29	36·5	45	46	S.	W.	calm	S.	·022	·20	...	...	36
7.	30·256	30·379	30·272	29·80	30·25	30·36	47·7	48·6	40·5	47	32	46	37½	35½	NW.	NW.	calm	WNW	·158	·09	·06	...	43
☾ 8.	30·362	30·461	30·376	29·98	30·45	30·45	38·8	48·7	37·6	44	26	33	36	34	NW.	N.	calm	SW.	·102	...	...	...	37
9.	30·412	30·441	30·381	30·03	30·37	30·29	33·8	41·0	32·9	35	25	34	35	33	NW.	NW.	calm	W.	...	...	...	...	33
10.	30·280	30·316	30·224	29·88	30·15	30·15	33·7	38·3	30·0	44	32	32	41	37	NW.	NW.	calm	W.	...	...	...	...	32
11.	30·288	30·407	30·321	29·87	30·30	30·28	39·9	40·6	33·7	45	35	39	27½	33	SW.	SW.	calm	NE.	...	...	...	...	34
12.	30·342	30·368	30·350	29·92	30·20	30·15	41·3	44·3	39·6	45	39	38.5	40	42	SW.	SW.	calm	SW.	...	...	...	0·07	36
13.	30·326	30·413	30·335	29·90	30·17	30·27	43·2	44·7	41·6	46	37	43	43	43	S.	W.	calm	SW.	...	...	...	...	38
14.	30·410	30·421	30·406	29·94	30·44	30·40	42·8	45·9	42·8	46	32	38	31	32½	NW.	N.	NW.	NE.	...	...	...	...	38
15.	30·366	30·375	30·346	29·97	30·32	30·25	40·6	46·2	41·0	45	28	36·5	35	40	ENE.	SE.	E.	SW.	...	...	...	...	38
16.	30·360	30·411	30·383	30·	30·29	30·35	38·4	43·3	38·2	38	31	26·5	37	40	E.	SE.	calm	S.	...	...	...	...	33
● 17.	30·366	30·396	30·355	29·95	30·32	30·30	35·6	39·8	35·4	37	33	36	37	33	SE.	N.	calm	SW.	...	...	...	...	34
18.	30·316	30·343	30·295	29·99	30·23	30·15	35·3	36·6	34·9	38	31	33·5	32	33	SSE.	NE.	calm	SE.	...	...	...	...	32
19.	30·224	30·257	30·202	29·85	30·05	29·95	34·6	36·3	33·4	42	26	32	31	34½	SSE.	SE.	W.	ESE.	...	...	...	0·08	30
20.	30·192	30·348	30·223	29·80	29·98	30·23	36·7	37·3	34·9	43	28	35	40	39	S.	SE.	W.	NW.	...	...	·06	...	32
21.	30·364	30·397	30·210	30·	30·25	30·02	35·8	36·7	35·7	38	30	36·5	40	36	SE.	SE.	calm	ESE.	...	...	...	...	35
22.	30·104	30·397	29·988	29·75	29·95	29·81	35·8	38·3	34·2	46	37	33	37	37	S.	SE.	SE.	ENE.	...	·30	·05	...	33
23.	29·572	29·593	29·386	29·23	29·39	29·32	42·8	43·7	36·0	46	42	42	39	44	E.	SE.	SE.	E.	·311	·03	·12	...	36
☽ 24.	29·310	29·478	29·312	29·05	29·43	29·55	41·8	44·4	41·8	43	31	40	40½	40	E.	SE.	SE.	E.	·047	·01	·03	...	37
25.	29·658	29·954	29·700	29·29	29·85	29·98	34·5	35·7	34·2	37	21	32	30½	29	NW.	N.	SE.	NE.	·016	...	...	...	32
26.	29·918	29·949	29·472	29·48	29·58	29·35	29·7	34·8	29·8	41	32	28	32½	36½	S.	S.	calm	SSW.	...	·20	...	1·80	27
27.	29·662	30·065	29·706	29·26	29·58	29·87	34·8	39·2	30·0	44	27	35·5	35	34	SW.	W.	calm	W.	·147	...	·34	...	31
28.	30·230	30·383	30·247	29·79	30·18	30·05	33·8	40·8	33·8	40	25	35	32	29½	W.	W.	calm	SSW.	...	...	...	...	31
29.	30·222	30·248	30·235	29·70	29·85	29·99	39·9	40·8	33·8	49	43	37	44½	42½	S.	SW.	W.	W.	...	·08	...	...	33
30.	30·152	30·313	30·169	29·52	29·68	30·05	47·4	48·2	40·2	51	32	48	45	35	S.	W.	W.	S.	·033	·05	...	...	38
⊙ 31.	30·496	30·601	30·487	29·91	30·34	30·34	36·8	50·7	36·2	44	30	36	36	40½	WNW.	W.	calm	WSW.	·044	...	...	...	35
Mean.	30·075	30·181	30·042	29·96	29·948	29·965	39·6	43·5	37·5	44·45	32·7	37·7	38	37·¾					Sum. 1·601	1·72	0·79	2·87	Mean. 35·7

THE

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[THIRD SERIES.]

MARCH 1839.

XXVII. *On the general Magnetic Relations and Characters of the Metals: Additional Facts. By Prof.* Faraday.*

An idea that the metals would be all magnetic if made extremely cold, as they are all non-magnetic if above a certain temperature, was put forth in March 1836†, and some experiments were made, in which several were cooled as low as −60° or −70° Fahr., but without acquiring magnetic powers. It was afterwards noticed ‡ that Berthier had said that besides iron, cobalt, and nickel, *manganese also possesses magnetic force beneath a certain degree of temperature, much below zero.* Having had last May the opportunity of working with M. Thilorier's beautiful apparatus for giving both the liquid and the solid state to carbonic acid gas, I was anxious to ascertain what the extremely low temperature procurable by its means would effect with regard to the magnetic powers of metals and other substances, especially with relation to manganese and cobalt; and not having seen any account of similar trials, I send the results to the Philosophical Magazine (if it please the Editors to insert them) as an appendix to the two former notices.

The substances were cooled by immersion in the mixture of ether and solid carbonic acid, and moved either by platina wires attached to them, or by small wooden tongs, also cooled. The temperature, according to Thilorier, would be about 112° below 0° of Fahrenheit. The test of magnetic power was a double astatic needle, each of the two constituent needles be-

* Communicated by the Author.

† London and Edinb. Phil. Mag., vol. viii. p. 177.

‡ *Ibid.*, vol. ix. p. 65.

ing small and powerful, so that the whole system was very sensible to any substance capable of having magnetism induced in it when brought near one of the four poles. Great care was required and was taken to avoid the effect of the downward current of air formed by the cooled body; very thin plates of mica being interposed in the most important cases.

The following metals gave no indications of any magnetic power when thus cooled to −112° Fahr.

Antimony,	Lead,
Arsenic,	Mercury,
Bismuth,	Palladium,
Cadmium,	Platinum,
Chromium,	Rhodium,
Cobalt,	Silver,
Copper,	Tin,
Gold,	Zinc.

A piece of metallic *manganese* given to me by Mr. Everett was very slightly magnetic and polar at *common* temperatures. It was not more magnetic when cooled to the lowest degree. Hence I believe the statement with regard to its acquiring such powers under such circumstances to be inaccurate. Upon very careful examination a trace of iron was found in the piece of metal, and to that I think the magnetic property which it possessed must be attributed.

I was very careful in ascertaining that pure *cobalt* did not become magnetic at the very low temperature produced.

The native alloy of iridium and osmium, and also crystals of titanium, were found to be slightly magnetic at common temperatures; I believe because of the presence of iron in them.* Being cooled to the lowest degree they did not present any additional magnetic force, and therefore it may be concluded that *iridium*, *osmium*, and *titanium* may be added as non-magnetic metals to the list already given.

Carbon and the following metallic combinations were then experimented upon in a similar manner, but all the results were negative: not one of the bodies gave the least sign of the acquirement of magnetic power by the cold applied.

1. Carbon.	7. Native oxide of tin.
2. Hæmatite.	8. ——— ——— manganese.
3. Protoxide of lead.	9. Chloride of silver.
4. ——— antimony.	10. ——— lead.
5. ——— bismuth.	11. Iodide of mercury.
6. White arsenic.	12. Galena.

[* See Dr. Wollaston's paper on this subject, Phil. Trans. 1823, Part II., or Phil. Mag. First Series, vol. lxiii. p. 15. Edit.]

13. Realgar.
14. Orpiment.
15. Dense native cinnabar.
16. Sulphuret of silver.
17. ———— copper.
18. Sulphuret of tin.
19. ———— bismuth.
20. ———— antimony.
21. Protosul. iron crystallized.
22. ———— anhydrous.

The carbon was the dense hard kind obtained from gas retorts; the substances 3. 4. 5. 6. 9. 10. 11. and some of the sulphurets had been first fused and solidified; and all the bodies were taken in the most solid and dense state which they could acquire.

It is perhaps superfluous to add, except in reference to effects which have been supposed by some to occur in northern latitudes, that the iron and nickel did not appear to suffer any abatement of their peculiar power when cooled to the very lowest degree.

Royal Institution, Feb. 7, 1839.

XXVIII. *Notice on the Theory of the Æthers.* *By* ROBERT KANE, *M.D., Professor of Chemistry in Dublin.**

IN discussing the claims advanced by Robiquet and Boutron, to the discovery of the important relations of amygdaline and oil of bitter almonds, Professor Liebig has introduced my name in such a manner as to render a notice of it upon my part unavoidable. Robiquet's memoir and Liebig's criticism on it are in the *Annalen der Pharmacie* for February 1838, which, however, did not reach Dublin until October, and the passage in question (page 195) is as follows:

"Es kommt mir gerade so vor, als wenn Hr. Kane, Berzelius gegenüber, behaupten wollte, er sey der Entdecker der Aethyl-theorie, die ihm abgeschmackter-weise von einigen seiner Landsleute zugeschrieben wird, weil er einmal die Idee hatte, dass man den Aether auch wohl als ein Oxyd betrachten könne, eine Idee, die mit eben so viel Recht von Hrn. Dumas und Boullay in Anspruch genommen wird. Ich wündere mich nur darüber, dass er seine Ansprüche nicht ganz ehrlich desavouirt; denn während des Kampfes hat er sich in den hintersten Reihen gehalten, ja er ist so gar von der einen Seite zur andern übergegangen, und sicher denkt er nicht daran, jetzt, wo der Sieg entschieden ist, sich die Lorbeeren anzueignen."

In order that the nature of the matter under consideration may be intelligible to every person, I will attach a translation of the passage.

* Communicated by the Author.

"It appears to me exactly as if Mr. Kane wished to assert, in opposition to Berzelius, that he is the discoverer of the Ethyl theory, which some of his countrymen, in an absurd manner ascribe to him, because he at one time had the idea that æther might as well be considered as an oxide, an idea which with just as much right has been laid claim to by Dumas and Boullay. I am only astonished that he has not honourably, totally disavowed his claims, since during the battle he has kept himself in the rearmost ranks, nay, he has passed over from one side to the other; and certainly he cannot think, now that the victory has been decided, to appropriate the laurels to himself."

In the observations which it is my duty to make, in order to show the groundlessness and unreasonable nature of the remarks of Professor Liebig, I am anxious that it should be understood that I do not mean to enter into any personal or recriminating discussion with a philosopher to whom I am under so many obligations for the kindness and zeal with which he directed my first studies in organic chemistry in his laboratory at Giessen, and with whom I continue up to the present day in constant and most friendly communication. It is my duty to show that my theory of the æthereal combinations was not merely an accidental idea or a vague point of view, but that from the commencement, it possessed a completeness and accordance with facts, which the system of Berzelius did not acquire until it had undergone an important modification in the hands of Professor Liebig.

In January 1835 I published in the Dublin Journal of Medical and Chemical Science a short paper, in which the question of priority between Berzelius and myself was discussed and the exact nature and extent of my views fully shown. Neither that paper nor the original notice of 1833 was ever copied into any English scientific journal, nor, that I am aware of, into any foreign one. In 1836 I asked one of the Editors of the Philosophical Magazine to notice it; but it was not done, probably because the chemists in England did not take much interest in such subjects at the time. Neither Dr. Turner in his Elements of Chemistry, nor still later Dr. Thomson in his Organic Chemistry, has made any reference to my memoir, although both giving the Ethyl theory, and ascribing the discovery of it to Berzelius and Liebig; in fact the only one of my countrymen who to my knowledge is exposed to the charge of bad taste in alluding to my views, is Mr. Richard Phillips, who in his translation of the London Pharmacopœia has noticed the priority of my paper, and has consequently given me credit for it.

In place of entering afresh into the discussion of priority, I would ask the Editors of the Philosophical Magazine to print the following quotation, from the Dublin Journal of Medical and Chemical Science, vol. ii. p. 348. January 1833. My claim will then go before the chemical world, which it has never yet done; it can be judged of fairly, and I shall be quite satisfied to abide by their decision; but my disavowing the discovery of what Liebig himself calls one of the most important theories in chemistry, when I believe the facts to be most positively on my side, would certainly be much less honourable than the course which Liebig blames me for having adopted.

The idea of Dumas and Boullay to which Liebig alludes, was simply that æther itself might be looked upon as a base. So may the vegetable alkalies: but neither Liebig nor any other chemist would think of generalizing the ethyl theory, and, in the present state of science, considering morphia, strychnia, quinia, &c. as oxides of peculiar radicals. Consequently Dumas's idea did not contain even the germ of the ethyl theory; whilst I brought forward that theory more completely formed, even than Berzelius did. No person can respect more profoundly the extent and accuracy of view which has throughout his long career so much distinguished the Swedish chemist than I do; I seek not to diminish his merit in proposing, nor that of Liebig in improving, the theory which is now made the subject of dispute. I never called in question or ever doubted of the perfect originality and independence of his views; but that the theory was original with me, that in time my memoir preceded that of Berzelius, and that my views were the same as those now held by Liebig and Berzelius, are matters of fact which I could not disavow without being guilty of a falsehood.

Liebig's remark, that during the battle I have kept in the rearmost ranks, requires from me a few words of explanation. In the first place, I have no abstract love for fighting, particularly when it must be amongst friends, who, whatever they may become less, are never more friendly after than they were before it. My memoirs were published; I left them to sink or swim, the opinions to be adopted or not, accordingly as time and examination should test their value. Perhaps I was wrong in not making more noise about my theory, but at that time my speculations about the æthers were as regularly a subject of amusement and ridicule among the chemical circles in Dublin as my analyses of white precipitate were a year or two ago, and as my theory of the ammoniacal combinations is now; so I contented myself with setting my little skiff afloat

and taking my chance for the rest. In fact, I still think the matter of very little importance; and were it not that my silence might be wrongly interpreted by evil-minded persons, of whom this world does contain a few, I should have been content to see without a murmur the ethyl theory ascribed to Berzelius as long as it remains in chemistry, which I am very much disposed to think will not be long; as yet, however, it is the best we have.

Setting out from the ammonium theory of Berzelius, I developed the theory of the æthers, assuming æthereum (ethyl) as a compound radical. Three years' continued labour at the ammoniacal combinations have given me a view of their nature, which I hope the Royal Irish Academy will soon have printed. In the mean time abstracts of my numerical results and of my views have been published in the Proceedings of the Academy.* This ammonia-theory modifying my views of the nature of compound radicals, made me reconsider the views of the æthereal combinations, and I declared myself to Liebig when he was in Dublin as undecided about the constitution of the æthers; and being disposed to consider my results about the ammoniacal combinations as likely to affect considerably the existing theories of the æthers, I recollect perfectly an expression which, though trivial, perhaps Liebig himself may yet bear in mind. I said to him, that I had discarded all æther theories, and that my ideas about them were all mashed up together. He said, Let them ferment and we shall see what will come out of it. This was trifling talk, but it will be seen that his observation that I had gone from one side to the other is by means of these facts deprived of the force which, in an evil point of view, it might at first sight be supposed to possess.

With regard to the laurels, I have also a word to say. There are two very different, but both important phases in the history of the ethyl theory; the first its proposition, the second its (*pro tanto*) establishment. Anything in the way of a leaf for the first, I would certainly lay claim to equally with Berzelius; but for the second, to which I consider the greater part of the branch should be devoted, no person can dispute Liebig's right. When I proposed the theory, I did so from an examination of the results of others. I had then never made an organic analysis; I never made an organic analysis until Liebig showed me how in Giessen. Neither did Berzelius deduce the theory from his own observations: the analyses by Magnus, by Liebig, and by Mar-

[* Some of Prof. Kane's results on the ammoniacal combinations will be found in L. & E. Phil. Mag. vol. xiii. p. 156. Edit.]

chand gave Berzelius the materials: I am sure, if neither Berzelius nor myself had in that year proposed the theory, Liebig or somebody else would have done it in the next; science was ripe for it, and it could not have been left undone.

Since the proposition of the theory by Berzelius and myself, the field has been left in Liebig's possession almost completely; he has fully established the superiority of the ethyl theory over the old views of Dumas and Boullay, and has shown that in the present state of science it alone fulfils the conditions of a sufficient theory. But what is the present state of science? I believed in the sufficiency of the ammonium theory three years ago, and proposed the ethyl theory after its model. I have satisfied myself that we can have in the present state of science a theory still better than the ammonium one of Berzelius for the compounds of ammonia, and I hope that science will advance so rapidly as to render very soon the ethyl theory insufficient; and I have myself only refrained from a full examination of the æthereal compounds with reference to applying to them the principles of my theory of ammonia, because I am anxious to devote more time to a subject of so much importance than I have at present at my own disposal. I have already written to Professor Liebig, and he has printed a few words on the change which my views of ammonia may make in the theory of the æthers, and I shall proceed to their development as soon as ever I can find time.

"*Theory of the Æthers**.

"Dumas and Boullay had determined that in the æthers the carburetted hydrogen might be regarded as a base similar to ammonia; they even contrasted in a table its properties to those of ammonia, and showed that in all the important characteristics it was equally marked, and that but for the accidental circumstance of its insolubility in water, its alkaline nature should have been long since recognised. Having devoted some attention to the ammonium theory of Berzelius, in which he regards an atom of hydrogen as converting the ammonia into a substance possessing many properties in common with the metals, I was induced to try whether the same simplicity of arrangement and classification which was given to the ammonia compounds by that hypothesis, could not be afforded to the different combinations of the æthers by the assumption of similar principles. Let us consider the base

* Dublin Journal of Medical and Chemical Science, vol. ii. p. 348.

of the æthers as being, not olefiant gas, but, as Thomson proposed, the isomeric liquid, whose formula is $(4\,C + 4\,H)$; denote by the name of *ethereum* the hypothetic body formed by its union with an atom of hydrogen, (as Berzelius terms the compound of ammonia + an atom of hydrogen, *ammonium*;) and see the expressions for the composition of some of the most interesting of these bodies.

Sulphuric æther (oxide of æthereum) $= (4\,C + 4\,H) + H + O.$

Alcohol (hydrated oxide of æthereum) $= (4\,C + 4\,H) + \dot{H} + O + H.$

Muriatic æther (chloride of æthereum) $= (4\,C + 4\,H) + (H + Ch.)$

Hydriodic æther (iodide of æthereum) $= (4\,C + 4\,H) + (H + I.)$

Nitrous æther (hypo-nitrite of oxide of æthereum) $= \dddot{N} + (4\,C + 4\,H) + (H + O).$

Oxalic æther (oxalate of oxide of æthereum) $+ 2\,\dddot{C} + (4\,C + 4\,H) + (H + O).$

"Any one conversant with the subject will at once see how simply the above view accounts for the varied decompositions which occur in the production of these different bodies. I regret that the necessary brevity of this note prevents me from illustrating any instance in detail, for it would facilitate very much the comprehension of the subject. It is at once apparent that the different oxy-combinations of æthereum have been well studied, and that it is very probable that corresponding chlorine, iodine, &c., compounds exist, a few of which, as muriatic, hydriodic, and hydro-sulphocyanic æthers, are already known. I had intended to enter into the development of this subject myself, but want of time prevented me; the only experiments I made on it are a few, which I shall subsequently relate. I now bring the subject forward in order to direct the attention of those persons who are interested in the progress of chemical philosophy to it, that its truth or falsity may be, if possible, proved."

XXIX. *Researches in the Undulatory Theory of Light, continued; on the Elliptical Polarization produced by Quartz. Part* I. *By* J. TOVEY, *Esq.*

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

AT the conclusion of my paper on the Cause of the Elliptical Polarization of Light, (L. and E. Phil. Mag., vol. xii. p. 10,) I stated that I intended to apply my formulæ to the case of this phænomenon produced by quartz crystal. The fulfilment of this intention has been long delayed; but I now proceed to the subject.

In the first place it is needful to put the last four of the expressions (3.), of the paper referred to, into a more convenient form, thus: by the rules of trigonometry,

$$1 - \cos k\,\Delta x = \text{vers}\, k\,\Delta x,$$

and

$$\begin{aligned}
\cos(k\,\Delta x - b) - \cos b &= -\cos b \,\text{vers}\, k\,\Delta x + \sin b \sin k\,\Delta x,\\
\cos(k\,\Delta x + b) - \cos b &= -\cos b \,\text{vers}\, k\,\Delta x - \sin b \sin k\,\Delta x,\\
\sin(k\,\Delta x - b) + \sin b &= +\sin b \,\text{vers}\, k\,\Delta x + \cos b \sin k\,\Delta x,\\
\sin(k\,\Delta x + b) - \sin b &= -\sin b \,\text{vers}\, k\,\Delta x + \cos b \sin k\,\Delta x;
\end{aligned}$$

hence if we put

$$\left.\begin{aligned}
m\,\Sigma.\,\psi(r)\,\Delta y\,\Delta z \,\text{vers}\, k\,\Delta x &= \sigma,\\
m\,\Sigma.\,\psi(r)\,\Delta y\,\Delta z \,\sin\, k\,\Delta x &= \sigma',
\end{aligned}\right\} \quad (10.)$$

the expressions referred to will become

$$\left.\begin{aligned}
-\sigma \cos b + \sigma' \sin b &= s_2,\\
-\sigma \cos b - \sigma' \sin b &= s'_2,\\
\sigma' \cos b + \sigma \sin b &= s_3,\\
\sigma' \cos b - \sigma \sin b &= s'_3.
\end{aligned}\right\} \quad (11.)$$

Now the third and fourth of the equations (4.)* give $s_, s'_, = s_3 s'_3$; hence $s_, s'_, = \sigma'^2 \cos^2 b' - \sigma^2 \sin^2 b$, and therefore

$$\cos b = \pm \sqrt{\frac{s_, s'_, + \sigma^2}{\sigma'^2 + \sigma^2}}. \quad (12.)$$

The third of the equations (4.), and the value of s'_3 in (11.) give

$$\rho = -\frac{s_,}{\sigma' \cos b - \sigma \sin b}. \quad (13.)$$

Let cos b and $-\cos b$ denote the two values of cos b given by (12.); these values will afford, by (13.), two corresponding values of ϱ, which denote by $\varrho_,$ and ϱ_2. Now either pair of these values satisfies the conditions of the problem; hence

* The equations numbered from (1.) to (9.) inclusive, are in the former paper.

the first and second of the equations (4.) give, by taking the values of s_2, s'_2, from (11.)

$$\left.\begin{aligned} &n^2 + s - \rho_1 (\sigma \cos b + \sigma' \sin b) = 0, \\ &n^2 + s + \rho_2 (\sigma \cos b - \sigma' \sin b) = 0, \\ &n^2 + s' - \frac{\sigma \cos b - \sigma' \sin b}{\rho_1} = 0, \\ &n^2 + s' + \frac{\sigma \cos b + \sigma' \sin b}{\rho_2} = 0: \end{aligned}\right\} \quad (14.)$$

because the sign of the sine of an arc remains the same while that of the cosine changes.

For the sake of abridgement put

$$\left.\begin{aligned} &\phi(r) + \psi(r) \Delta y^2 = p, \\ &\phi(r) + \psi(r) \Delta z^2 = p', \\ &\psi(r) \Delta y \Delta z = q, \end{aligned}\right\} \quad (15.)$$

$$\begin{aligned} &m \; \Sigma . p \Delta x = A, && m \Sigma . p' \Delta x = A', \\ &\frac{m}{2} \Sigma . p \Delta x^2 = A_1, && \frac{m}{2} \Sigma . p' \Delta x^2 = A'_1, \\ &\frac{m}{2.3} \Sigma . p \Delta x^3 = A_2, && \frac{m}{2.3} \Sigma . p' \Delta x^3 = A'_2, \\ &\frac{m}{2.3.4.} \Sigma . p \Delta x^4 = A_3, && \frac{m}{2.3.4} \Sigma . p' \Delta x^4 = A'_3, \\ &\&c. && \&c. \end{aligned} \quad (16.)$$

$$\begin{aligned} &m \; \Sigma . q \Delta x = B, \\ &\frac{m}{2} \Sigma . q \Delta x^2 = B_1, \\ &\frac{m}{2.3} \Sigma . q \Delta x^3 = B_2, \\ &\&c. \end{aligned}$$

Then, since

$$\sin k \Delta x = k \Delta x - \frac{k^3 \Delta x^3}{2.3} + \&c.,$$

$$\cos k \Delta x = 1 - \frac{k^2 \Delta x^2}{2} + \frac{k^4 \Delta x^4}{2.3.4} - \&c.,$$

$$\text{vers } k \Delta x = 1 - \cos k \Delta x,$$

the formulæ (3.) and (10.) give

$$\left.\begin{aligned} s &= - A_1 k^2 + A_3 k^4 - \&c. \\ s' &= - A'_1 k^2 + A'_3 k^4 - \&c. \\ s_1 &= A k - A_2 k^3 + \&c. \\ s'_1 &= A' k - A'_2 k^3 + \&c. \\ \sigma &= B_1 k^2 - B_3 k^4 + \&c. \\ \sigma' &= B k - B_2 k^3 + \&c. \end{aligned}\right\} \quad (17.)$$

Before we proceed further we must make a few observations on the nature and relative magnitudes of the quantities contained in the formulæ (17.) The capital letters denote quantities depending entirely on the nature of the medium; and by examining (15.) and (16.) we may see that $A_{,}$ and A_3 are sums of which the terms are all positive; but we infer that the latter is almost indefinitely smaller than the former, because it involves a power of Δx higher by two units, and Δx can never exceed the radius of the sphere of influence of a molecule of the medium. The same observations apply to $A_{,}'$ and A'_3. The quantities A, A_2, ... A', A'_2, ... B, $B_{,}$, B_2, ... are sums in each of which the terms must be about half of them positive and half negative, hence these quantities must be comparatively small, and their real magnitudes can be learnt only by comparing with experiments the results of the calculation, the basis of which must originally be hypothetical.

The sum $B_{,}$, which is the principal quantity in the value of σ, is the sum which we have shown in art. 15, p. 426, vol. ix. can always be made to vanish, by taking the axes of y and z in proper directions. Let us suppose this done, and that the other sums in σ are indefinitely small in comparison with $A_{,}$, $A_{,}'$. Let us also suppose $\frac{B}{k}$ to be insensible. Then, by substituting the values (17.) in the equation (14.), writing v for $\frac{n}{k}$, transposing, and stopping at the terms explicitly expressed in (17.), we get

$$\begin{aligned} v_{,}^2 &= A_{,}-A_3\,k_{,}^2-\varrho_{,}\sin b\,.\,B_2\,k_{,}, \\ &= A_{,}'-A_3\,k_{,}^2+\frac{\sin b\,.\,B_2\,k_{,}}{\rho_{,}} \\ v_2^2 &= A_{,}-A_3\,k_2^2-\rho_2\sin b\,.\,B_2\,k_2 \\ &= A_{,}'-A'_3\,k_2^2+\frac{\sin b\,.\,B_2\,k_2}{{}_{,}\,\rho_2}: \end{aligned} \tag{18.}$$

where we have marked v and k with subscript figures, because the values of these quantities must necessarily be different in the two equations, while that of n remains the same.

Suppose σ^2, and $\sigma\sin b$, to be indefinitely small in comparison with the quantities to which they are joined in (12.) and (13.), then

$$\cos b = \pm\sqrt{\frac{s_{,}\,s'_{,}}{\sigma'^2}}, \tag{19.}$$

and

$$\rho = -\frac{s_{,}}{\sigma'\cos b}; \tag{20.}$$

hence, if we suppose $s_{,} = s'_{,}$, the two values of ρ will be $\rho_{,} = -1$ and $\rho_2 = 1$; and, assuming $\sqrt{\frac{s_{,} s'_{,}}{\sigma'^2}}$ to be a very small quantity, we shall have $\sin b = 1$ and $b = \frac{\pi}{2}$, nearly. Now, since $k = \frac{2\pi}{\lambda}$, where λ is the length of a wave, if these values be substituted in (18.), and the terms involving A_3, A'_3, be comparatively insensible, there will result

$$v_{,}^2 = A_{,} + B_2 \frac{2\pi}{\lambda_{,}},$$
$$v_2^2 = A_{,} - B_2 \frac{2\pi}{\lambda_2}. \qquad (21.)$$

Since $\frac{n}{k} = v$, and $k = \frac{2\pi}{\lambda}$, the expressions (2.) may be changed to

$$\eta = a \sin\left\{\frac{2\pi}{\lambda}(vt - x)\right\},$$
$$\zeta = \rho a \sin\left\{\frac{2\pi}{\lambda}(vt - x) - b\right\}.$$

Now either of the values of v, and the corresponding value of ϱ, may be substituted for v and ϱ in these expressions. But since the equations (1.) are of the first degree, they may be satisfied not only by the values of η and ζ corresponding to each value of v, but by taking for η and ζ the sums of these particular values, in which we may change the value of a as v changes. Hence the equations (1.) may be satisfied by

$$\eta = a_{,} \sin\left\{\frac{2\pi}{\lambda_{,}}(v_{,}t - x)\right\} + a_2 \sin\left\{\frac{2\pi}{\lambda_2}(v_2 t - x)\right\} \qquad (22.)$$
$$\zeta = \rho_{,} a_{,} \sin\left\{\frac{2\pi}{\lambda_{,}}(v_{,}t - x) - b\right\} + \rho_2 a_2 \sin\left\{\frac{2\pi}{\lambda_2}(v_2 t - x) - b\right\}.$$

If in these expressions we give to $\rho_{,}$, ρ_2, and b, the values just assigned to them, namely, -1, 1, and $\frac{\pi}{2}$, we shall have

$$\eta = a_{,} \sin\left\{\frac{2\pi}{\lambda_{,}}(v_{,}t - x)\right\} + a_2 \sin\left\{\frac{2\pi}{\lambda_2}(v_2 t - x)\right\},$$
$$\zeta = -a_{,} \cos\left\{\frac{2\pi}{\lambda_{,}}(v_{,}t - x)\right\} + a_2 \cos\left\{\frac{2\pi}{\lambda_2}(v_2 t - x)\right\}. \qquad (23.)$$

Now suppose a quartz crystal to have two parallel faces perpendicular to its axis. Take x in the direction of the

axis; and conceive a ray of light, polarized in a given plane, to fall on the crystal in the direction of x; causing a vibration of the molecules situated in the surface of the crystal, which may be represented by

$$\eta = a \sin . \frac{2\pi}{\lambda} v\, t : \qquad (24.)$$

where λ and v are the length and velocity of the waves in the incident ray.

Suppose the origin of x to be at the surface of the crystal on which the light falls: then, when x is zero, the expressions (23.) must coincide with (24.), which they will do if we make,

$$a_{\prime} = a_2 = \frac{a}{2}, \quad \frac{v_{\prime}}{\lambda_{\prime}} = \frac{v_2}{\lambda_2} = \frac{v}{\lambda}. \qquad (25.)$$

Let $\varepsilon = \sqrt{(\eta^2 + \zeta^2)}$, then ε is the actual displacement of a molecule within the crystal; and if we denote by α the angle which ε makes with η, then

$$\tan \alpha = \frac{\zeta}{\eta};$$

therefore, since $a_{\prime} = a_2$, the expressions (23.) give

$$\tan \alpha = \frac{\cos . \frac{2\pi}{\lambda_2}(v_2 t - x) - \cos . \frac{2\pi}{\lambda_{\prime}}(v_{\prime} t - x)}{\sin . \frac{2\pi}{\lambda_2}(v_2 t - x) + \sin . \frac{2\pi}{\lambda_{\prime}}(v_{\prime} t - x)}$$

$$= \tan . 2\pi x \left(\frac{1}{\lambda_{\prime}} - \frac{1}{\lambda_2}\right);$$

and hence
$$\alpha = 2\pi x \left(\frac{1}{\lambda_{\prime}} - \frac{1}{\lambda_2}\right).$$

Now the equations (21.) give us

$$\left.\begin{aligned} v_{\prime} &= A_{\prime}^{\frac{1}{2}} + \frac{B_2}{A_{\prime}^{\frac{1}{2}}} \frac{\pi}{\lambda_{\prime}}, \\ v_2 &= A_{\prime}^{\frac{1}{2}} - \frac{B_2}{A_{\prime}^{\frac{1}{2}}} \frac{\pi}{\lambda_2}; \end{aligned}\right\} \qquad (26.)$$

nearly: therefore, since $\frac{v_{\prime}}{\lambda_{\prime}} = \frac{v_2}{\lambda_2}$, we find

$$\frac{A_{\prime}^{\frac{1}{2}}}{\lambda_{\prime}} + \frac{B_2}{A_{\prime}^{\frac{1}{2}}} \frac{\pi}{\lambda^2_{\prime}} = \frac{A_{\prime}^{\frac{1}{2}}}{\lambda_2} - \frac{B_2}{A_{\prime}^{\frac{1}{2}}} \frac{\pi}{\lambda_2^2};$$

hence
$$\frac{1}{\lambda_{\prime}} - \frac{1}{\lambda_2} = -\pi \frac{B_2}{A_{\prime}^{\frac{1}{2}}}\left(\frac{1}{\lambda^2_{\prime}} + \frac{1}{\lambda_2^2}\right)$$

$$= -2\pi \frac{B_2}{A_{\prime}^{\frac{1}{2}}} \frac{1}{\lambda^2}, \text{ nearly;}$$

which gives

$$\alpha = -4\,\pi^2 \cdot \frac{B_2}{A_{\prime}^{\frac{1}{2}}} \cdot \frac{x}{\lambda^2}. \qquad (27.)$$

From this result we learn, 1st, that since α is the same for all values of t, every molecule vibrates to and fro in a straight line; 2nd, that α varies as $\frac{x}{\lambda^2}$, that is, directly as the thickness of the crystal through which the ray has passed, and inversely as the square of the length of the wave.

These results are equivalent to the well-known experimental laws of Biot; hence we conclude that our formulæ (23.), (25.), (26.), (27.), represent correctly the motions which constitute a ray of light passing through the crystal in the direction of its axis.

On looking at the length of this paper I think it will be best to reserve the remainder of the investigation for another.

I am, Gentlemen, yours, &c.

Littlemoor, Clitheroe, Jan. 9, 1839. JOHN TOVEY.

P.S. In the paper, vol. xii. p. 11, line 8, *for* $\phi(r)$ *read* $\phi(r)\,\Delta\eta$; *ibid.* line 9, *for* $\phi(r)$ *read* $\phi(r)\,\Delta\zeta$; and p. 12, equations (7.) *for* $\rho_{\prime\prime}\,a_{\prime\prime}\,(n_{\prime\prime}t - k\,x - b)$, *read* $\rho_{\prime\prime}\,a_{\prime\prime}$ sin $(n_{\prime\prime}t - k\,x - b)$. The corrections of a few other errors in the same paper have been previously given.

XXX. *On a remarkable Property of Arteries considered as a Cause of Animal Heat.* *By* J. M. WINN, *M.D.**

ABOUT three years since whilst making a few experiments with caoutchouc, I was forcibly struck with the property it possesses of evolving heat when suddenly stretched, and was led at the time to infer the probability of other bodies being similarly endowed. The elastic coat of arteries especially, from the mechanical resemblance it bears to caoutchouc, appeared to be one of the substances most likely to exhibit this calefactory principle; and in the event of this being the case it would not be unreasonable to conclude that the incessant contractions and dilatations of the arteries during life must prove an efficient source of animal heat.

During the past week I was induced to resume the subject afresh, and upon making an experiment with part of the aorta of a bullock, I felt much gratification in being able to verify my previous conjecture. The experiment was per-

* Communicated by the Author.

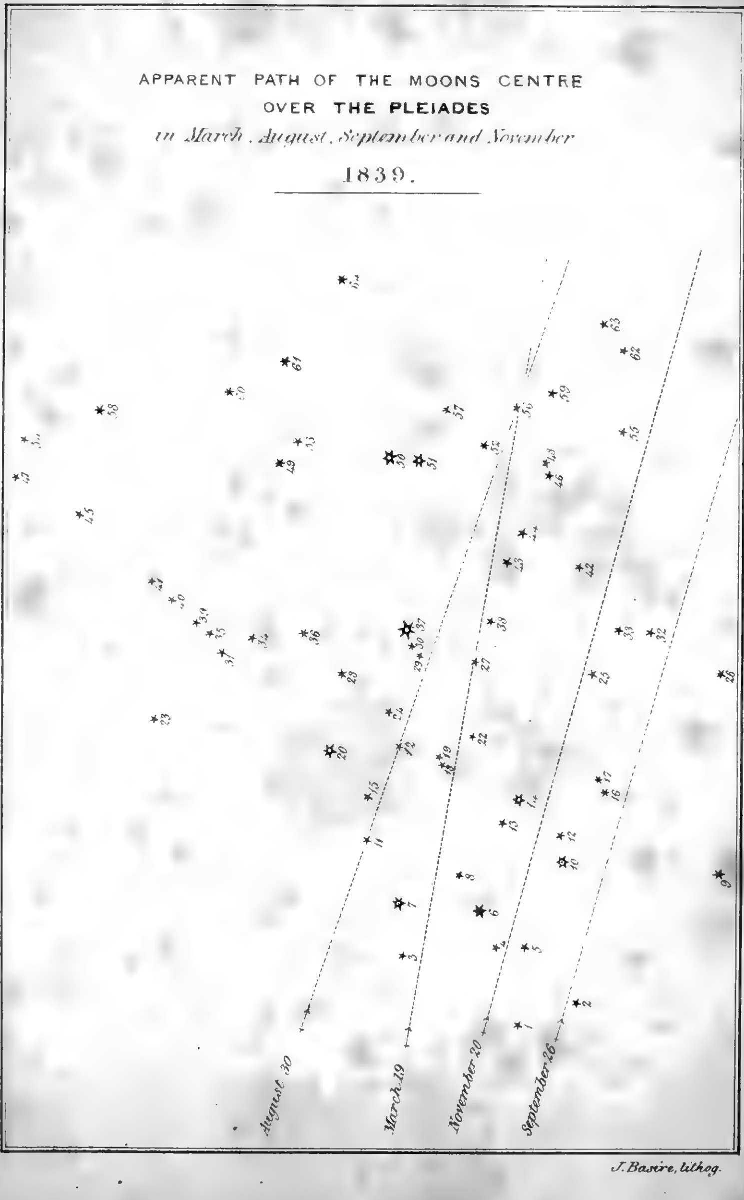
formed in the following manner. Having cut off a circular portion of the descending arch of the aorta, about an inch in length, I laid it open and carefully dissected out the elastic coat, and taking hold of it by each extremity, I pulled it to and fro with a continuous jerking motion (in imitation of the systole and diastole of the artery) for the space of about a minute, when placing it upon the bulb of a thermometer, I had the satisfaction to find that after it had remained two minutes the mercury had risen as many degrees. On removing the thermometer the heat immediately began to diminish. To be certain that the heat did not arise from any other source than the one in question, I took the precaution of covering my fingers with a double layer of flannel, to prevent the communication of heat from the body: I also covered my mouth with a handkerchief, to guard against the warm breath affecting the thermometer, whilst watching the progress of the experiment. I may likewise state that the experiment was performed in a room without a fire, the temperature of the air at the time being 55°. There were several difficulties to contend with during the investigation, and it was not until after repeated trials that the experiment succeeded to my satisfaction. The chief impediment I think must have been owing to the moisture of the artery, which by its evaporation must have had a constant tendency to carry off the heat. Having however performed the experiment twice consecutively in the same satisfactory manner, I think there can be but little doubt entertained as to its conclusiveness. My attention was often arrested, whilst conducting the experiments, by the striking mechanical analogies between caoutchouc and the elastic coat of arteries. Like the latter it could be elongated to twice its ordinary length, and, on withdrawing the tension, would return to its usual dimension with considerable force and a snapping noise. I was also surprised to find, on slightly drying it, that it would erase black-lead pencil marks from paper without leaving a stain. This latter circumstance is perhaps of trifling importance; it serves however to show that strong mechanical resemblance may exist between bodies widely differing in their chemical properties.

From the foregoing observations I think I am entitled to conclude that the whole of the heat developed in the animal œconomy can now be satisfactorily explained. Physiologists have often proved that the greater part of animal heat is occasioned by the chemical changes which take place in the lungs during respiration; there always remained however a portion which could not be referred to that source, but which can now I consider be fully accounted for by the mechanical

action of the arteries. The precise quantity of heat given off during each beat of an artery, it would be exceedingly difficult, perhaps impossible, to discover; but if we admit the development of only a very small quantity, it necessarily follows, from the circumstance of the action of the arteries being in incessant operation during life, that the heat must quickly accumulate to a great extent, and that the body unless cooled by the functions of the skin and lungs, would in a short space of time become preternaturally hot.

The following physiological and pathological facts appear to corroborate the view I have taken of the mechanical source of heat. 1st, The minute distribution of the arteries to every part of the system ensures a general and equal distribution of heat. 2ndly, The ossification of the arteries in old age, by diminishing their elasticity, is a probable cause of the diminution of animal heat at the close of life. 3rdly, The increased warmth of the body from exercise appears to be more readily explicable upon the principle of increased force in the arteries rather than that of increased vigour in the functions of the lungs, in as much as the immediate effect of exercise is evidently to embarrass the breathing, as shown by the hurried respiration. 4thly. In many diseases of the lungs where its functions are all at fault at a time when the arteries are beating with increased violence, the heat of the body is found to be above the usual standard. 5thly, Medicines which diminish the contractility and elasticity of the arteries almost invariably reduce the heat of the body. 6thly, The heat of local inflammations, in cases where the constitution does not sympathize to any extent, cannot be easily referred to any other source, as the arteries immediately in the neighbourhood of the affected part are throbbing with violence at a time when its capillaries (which are supposed to play so large a share in the chemical theory of heat) are generally considered to be entirely arrested. Many facts of a similar nature could be enumerated, but enough I think have been stated to establish the truth of the theory in question.

Of the nature of the mechanical force I have been investigating little can be said; it may possibly be a kind of intermolecular friction. It is clearly, however, of a different nature from ordinary friction, and which has also been considered a cause of animal heat; but I think erroneously so, for on examining the mechanism of the human body we find that everywhere the most efficient means of defence have been provided against its effects, as seen in the various synovial, mucous, and serous membranes, &c. It is not the province, however, of the physiologist to speculate on the essential nature of me-



J. Basire, lithog.

chanical or vital forces. His legitimate object in the present state of the science would seem to be that of analysing the simplest operations in the human body:—to aim first at discovering the innumerable important processes that are carried on through the influence of physical agents, before he presumes to explain the higher and more mysterious principle of life: neither should he hastily call the vital power to his aid, to explain a phænomenon, such as heat, that is known to be common to every kind of matter, and which can be produced by a variety of physical forces totally independent of life.

Truro, Nov. 8, 1838.

XXXI. *On the Passage of the Moon across the Pleiades in March, August, September, and November* 1839. *By the Rev.* James Grooby, *F.R.A.S.*

[With a Chart: Plate V.]

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

THE annexed chart, showing the apparent path of the moon's centre over the Pleiades in the months of March, August, September, and November of the present year, might perhaps prove acceptable to some of your astronomical readers; but whether of sufficient importance to merit insertion in your next Number, I must leave to your determination. The occultation on the 19th of next month (March) will be particularly interesting; happening when the moon is only four days old, the whole of her disc will be visible; and as the immersions take place on the dark side, I have little doubt but that by excluding the enlightened part from the field of view, and using a telescope of considerable power, most, if not all, the telescopic stars which suffer occultation may be perceived, and their immersions observed. I should mention, that as astronomical telescopes, generally used in these observations, exhibit objects in an inverted order, the stars in the accompanying chart are inverted also, for the sake of more readily comparing them with the appearance in the telescope. In laying down the places of the stars, the right ascensions and declinations, as well as the numbers annexed to them, are taken from the catalogue of M. Jeaurat, as given by Mr. Baily in the Philosophical Magazine for September 1822, page 189, with the exception of No. 6, the declination of which as there given appears to be about 1′ 15″ too small. Mr. Baily in projecting his chart has made the degrees of right ascension

and declination the same length; I have here used the proportion of 55 to 60, which I believe is not far from the truth. If a circle of paper $1\frac{5}{32}$ inch in diameter be passed across the chart so that its centre may traverse the dotted line, it will readily be seen what particular stars the moon will cover, as well as the order in which they will disappear.

The Nautical Almanac gives the time of immersion, emersion and near appulse of eight of the principal stars, at Greenwich, as under:

At 8^h 6^m—No. 7 or *b* immerges.
8 9 —No. 6 or *g* immerges.
8 39 —No. 14 or *c* immerges.
8 44 —No. 10 or *e* immerges.
8 57 —No. 10 or *e* emerges.
9 4 —No. 7 or *b* emerges.
9 5 —No. 6 or *g* emerges.
9 9 near appulse of the moon's southern limb to No. 20 or *d*.
9 17 —No. 37 or *η* immerges.
9 28 —No. 14 or *c* emerges.
10 0 —No. 51 or *h* immerges, and No. 37 or *η* emerges.
10 17 near appulse of the moon's southern limb to No. 50 or *f*.
10 36 —No. 51 or *h* emerges.

Mr. Baily observes, that not only has the passage of the moon across the Pleiades at all times attracted the attention of astronomers, but that it is now more particularly interesting as affording a favourable opportunity for illustrating the theory of Cagnoli, with respect to his mode of determining the figure of the earth by means of occultations of the fixed stars. To which I will only add, that a more complete concurrence of favourable circumstances for this purpose can hardly be hoped for than that which the passage of the 19th of March presents. The small portion of the moon's disc which will be illuminated, her high northern declination, and her path so directed as to cover nearly the greatest number of stars possible, is a happy coincidence, which I am sorry to say we shall not meet with in any of the other passages of this year, and which, indeed, many years may pass before it occurs again. Wishing that we may be favoured with the only remaining, but most important desideratum, a cloudless sky,

I remain, Gentlemen, yours, &c.

Swindon, Feb. 8, 1839. JAMES GROOBY.

XXXII. *Meteorological Observations during a Residence in Colombia between the Years* 1820 *and* 1830. *By* Colonel Richard Wright, *Governor of the Province of Loxa, Confidential Agent of the Republic of the Equator, &c. &c.*

[Continued from p. 104.]

On the Method of Measuring Heights by Boiling Water.

IT will be observed in the following Journal, that the indication of *heights* is, in most cases, joined with that of boiling water. The former is in fact a deduction from the latter; I had but a confused idea of this method, till, upon my arrival at Quito, I met with a pamphlet of the late D. Francisco José Caldas (one of the most eminent victims sacrificed by the barbarity of Murillo on taking possession of Bogotà in 1816) published in 1819 at Bourdeaux, in which he details the steps by which he arrived at a knowledge of this principle, and the experiments by which he confirmed it. In the year 1801, during a scientific excursion in the neighbourhood of Popayan, he happened to break his thermometer; and in attempting to mend it he was led to observe the variability of the extremity of the scale corresponding to the heat of boiling water. His reflections on this subject led him, after various experiments, to the following conclusions: "The heat of boiling water is in proportion to the atmospherical pressure: the atmospherical pressure is in proportion to the height above the level of the sea; the atmospherical pressure follows the same law as the risings of the barometer, or, properly speaking, the barometer shows nothing more than the atmospherical pressure. Boiling water therefore shows it in the same manner as the barometer. It can consequently show the elevation of places in the same manner, and as exactly as this instrument." *Ensayo de una memoria sobre un nuevo metodo de medir las montañas, etc.* p. 10. His first experiment in Popayan gave b. w. $75^{\circ}\cdot 7$ of Reaumur, the height of the barometer being 22 in. 11 l. To find then the variation corresponding to one inch of the barometer:

$$28^{\text{in.}} - 22^{\text{in.}}\ 11^{\text{l.}} = 5^{\circ}\cdot 1 \text{ or } 61 \text{ lines.}$$
$$80^{\circ} - 75^{\circ}\cdot 7 = 4^{\circ}\cdot 3. \quad \text{Then}$$

$$61^{\text{l.}} : 4^{\circ}\cdot 3 :: 12^{\text{l.}} : \frac{4^{\circ}\cdot 3 \times 12}{61} = 0^{\circ}\cdot 8.$$

Then reversing the process

$$0^{\circ}\cdot 8 : 12^{\text{l.}} :: 4^{\circ}\cdot 3 : \frac{4^{\circ}\cdot 3 \times 12}{0^{\circ}\cdot 8} = 64\cdot 5 = 5^{\text{i.}}4\tfrac{1}{2}$$

Difference betwixt this result and that of the barometer $3\frac{1}{2}$ lines. Satisfied with this commencement, or dawning of a new theory, he began a series of experiments in the mountains near Popayan, taking this city as the centre of his labours, and fixing the elevation of the barometer at $22^{i\cdot}\ 11^{l\cdot}\ 2$, and boiling water at $75^{\circ}\cdot 65$ of Reaumur.

At a spot named Las Juntas I made my first observation. The barometer stood at $21^{i}\ 9^{l}$, or 14^{l} lower than at Popayan; the heat of boiling water was $74^{\circ}\cdot 5$ Reaumur. Then

Height of the barometer in Popayan	22^{i} 11·2	B.W.	$75^{\circ}\cdot 65$
at Las Juntas	21 9	———	$74^{\circ}\cdot 50$
	1 2·2		$1^{\circ}\cdot 15$

$$1\ 2{\cdot}2 = 14^{l}{\cdot}2 : 1^{\circ}{\cdot}15 :: 12^{l}\ \frac{12 \times 1^{\circ}{\cdot}15}{14{\cdot}2} = 0^{\circ}{\cdot}971 \text{ of Reaumur}$$

for $12^{l\cdot}$ of the barometer.

I ascended to Paisbamba, a small farm five leagues south of Popayan. Barometer $20^{i}\ 9^{l\cdot}1$. B.W. $73^{\circ\cdot}5$.

Barometer in Popayan	$22^{i}\ 11^{l\cdot}2$	B.W.	$75^{\circ}\cdot 65$
in Paisbamba	20 9 ·1	B.W.	73 ·50
Differences	2 2 ·1		2° 15

$$2\ 2{\cdot}1 = 26^{l\cdot}1 : 2^{\circ\cdot}15 :: 12\ \frac{12 \times 2{\cdot}15}{26{\cdot}1} = 0^{\circ\cdot}988 \text{ of Reaumur,}$$

for 12 lines of the barometer.

I ascended a hill E. of Paisbamba called Sombreros. Barometer $19^{i}.\ 6^{l\cdot}5$. B. W. $72^{\circ}\cdot 4$.

Barometer in Popayan	$22^{i}\ 11^{l\cdot}20.$	B. W.	$75^{\circ\cdot}65$
——— on Sombreros	19 6 ·05.	B. W.	72 ·40
Differences	3 5 ·15.		3 ·25

$$41^{l\cdot}15 : 3^{\circ\cdot}25 :: \frac{12 \times 3^{\circ\cdot}25}{41{\cdot}15} = 0{\cdot}947 \text{ for 12 lines barometer.}$$

I ascended the hill of Tambores: barometer $18^{i}\ 11^{l\cdot}6$. B. W. $71^{\circ\cdot}75$.

Barometer in Popayan	$22^{i}\ 11^{l\cdot}2.$	B. W.	$75^{\circ\cdot}65$
on Tambores	18 11 ·6.	B. W.	71 ·75
Differences	3 11 ·6		3 ·90

$$47^{l\cdot}6 : 3^{\circ\cdot}9 :: 12\ \frac{12 \times 3{\cdot}9}{47{\cdot}6} = 0{\cdot}983 \text{ for } 12^{l} \text{ barometer.}$$

Proof that above $\frac{9^{\circ}}{10}$ of Reaumur is the true exponent of one inch of the barometer.

I then proceeded to take the observations of Las Juntas and Sombreros, and calculating the exponent anew.

B. in Las Juntas	21	9	B. W.	74·60
in Sombreros	19	6·05		72·40
Differences	2	2·95		2·2

$26 \cdot 95 : 2^{\circ} \cdot 2 :: 12 \; \frac{12 \times 2 \cdot 2}{26 \cdot 95} = 0^{\circ} \cdot 979$ Reaumur for 12 lines of the barometer.

B. in Paisbamba	20	9·1.	B. W.	73°·50
in Tambores	18	11·6.		71 ·75
Differences	1	9·5		1°·75

$1 \cdot 9 \cdot 5 = 21^{l} \cdot 5 : 1^{\circ} \cdot 75 :: 12 \; \frac{12 \times 1^{\circ} \cdot 7}{21 \cdot 5} = 0^{\circ} \cdot 976$ of Reaumur for 12 lines of barometer.

The mean of the six quotients is 0·974, which may be assumed as the exact exponent of 12 lines of the barometer.

Given then the heat of boiling water in any place to find the corresponding elevation of the barometer, and consequently its height above the sea.

As 0°·974 : 12 lines, so is the difference of the heat of B. W. To ascertain at Popayan the number of inches, lines, &c. of the barometer. Ex. in Tambores, B. W. 71°·15, to find the corresponding height of the barometer.

B. W. in Popayan	75°·65
in Tambores	71 ·75
	3 ·90

$0 \cdot 974 : 12 :: \frac{3 \cdot 9 \times 12}{974} = 48^{l} \cdot 05 = 4 \cdot 0 \cdot 05.$

As Tambores is above Popayan, deduct this quantity from the height of the barometer in that city.

Bar. in Popayan	22	11·20	
Deduct	4	00·05	
Remain	18	11·15	ht. of bar. in Tambores.

Barometrical height observed	18	11·60
Do. by calculation of B.W.	18	11·15
Difference		45

a result as exact as can be desired.

Upon this principle I calculated the elevation of the following 11 places:

Popayan,	Poblason,
Juntas,	Buenavista,
Paisbamba,	Hevradura,
Sombreros,	Pasto,
Tambores,	Quito.
Estrellas,	

Memoria, &c. p. 13, *et seq.*

Working upon the foregoing principle, Caldas adapted to his thermometer a barometrical scale. The product of 0°·974 of Reaumur by 19 is 18·506, or, in round numbers 18·5, i. e. 18°·5 of Reaumur correspond to 19 inches of the barometer. Then measuring 18·5 from the summit, or 80° of Reaumur's scale, he transferred it to the opposite side of the thermometer, dividing it into 19 equal parts, or inches of the barometer, subdividing these by a nonius into 24 each = half a line of the barometer. In this manner the elevation of the thermometer by boiling water indicates the corresponding elevation of the barometer under the same atmospheric pressure. Caldas observes that Humboldt, to whom he had communicated these ideas, when they met in Popayan, objected the variability of the heat of boiling water under the same atmospherical pressure; to which he replies: "Long practice has taught me its invariability in this respect, using the requisite precautions in making the experiment: otherwise, how could there be equal thermometers? Is not the invariability of the heat of boiling water under the pressure of 28 inches the foundation of the superior term of all thermometrical scales? It is true that boiling water does not *immediately* acquire its extreme heat, but pushing the operation to its *maximum* its heat is always the same." p. 24.

Caldas did not consider an invariable exponent possible, on account of the variability of atmospheric pressure. The want, however, of a barometer induced me to make some experiments to this effect, by way of rendering this method of measuring elevations still more simple, and of more general use. Is the variability of atmospheric pressure such as to make any important difference in these calculations? Does not water boil constantly at 212° at the level of the sea? At Quito I found the same result as Caldas had several years before; and several times the same result in this and other parts of the Andes. The difference, then, is scarcely perceptible in the thermometer, and consequently unimportant in the results of a calculation founded on the heat of boiling water. The thermometer besides, immersed in boiling water, is less liable to a variety of atmospheric influences to which the mercury of the barometer is necessarily subject. Hence

the great differences in different barometrical measurements of the same elevations, and the differences observed betwixt different thermometers exposed to the air in the same place, which I have observed on comparing three together to amount often to $1\frac{1}{2}°$, and never to less than $\frac{1}{2}°$.

I took the following method to obtain an exponent of the value in feet of each degree of the diminished temperature of boiling water.

The elevation of Quito is, according to Boussingault, 9524; and water boils at 196°·25; 212° − 196°·25 = 15°·5. 9524 ÷ 15·75 = 604 ft. 6 in. nearly. Neglecting the fraction as unimportant, I assumed 604 feet for the value of the degree, and began my observation on the conical hill of Javirac, which backs the city, and is calculated at 729 feet in height. Water boiled here by two thermometers at 195°. Then 196°·25 ÷ 195 = 1·25, difference of boiling water between the hill and the city; and 1·25 × 604 = 755 feet; difference 26 feet. I next ascended the volcano of Pichincha, and found at the foot of the crater B. W. 186°. 212° − 186° = 26° × 604 = 15′ 730 feet; and adding 246 feet, the difference between this point and the summit, reckoned at 15′·976. There could be little error in the calculation. I next applied this formula to the heights of several places calculated by Humboldt, and where the heat of boiling water had been ascertained by Caldas.

Thus Bogota, height according to Humboldt	8694 ft.
B. W. according to Caldas 197°·6	8712
Difference	18
Popayan, according to Humboldt	5823
B. W. 202°·21	5922
Difference............	99
Pasto, according to Humboldt	8572
B. W. 197°·6	8712
	140 ft.

The differences here are in four points 27 feet, 18, 99, 140. With respect to the hill of Javirac, commonly called *El Panecillo*, I suppose the measurement to have been made by the Academicians. But their calculations generally differ from those of Humboldt, as in the case of Quito; the former giving 9371 feet, the latter 9537; Pichincha 15,606 feet, Humboldt 15,976; Chimborazo 20,583, Humboldt 21,414. But even a difference of sites is sufficient to account for the 27 feet on ground so unequal as that of Quito. The 18 feet

in the height of Bogotà is so trifling a difference, that it rather proves the exactness of my calculation. In Popayan we have 99 feet; yet the different barometrical measurements of that city differ still more widely. Caldas observes, p. 31, "The Baron de Humboldt's barometer stood in Popayan at 23 3·4, mine at 22 11·2, and Bouguer's at 22 10·7." The most accurate measurements of the peak of Teneriffe, selecting 4 out of 14, leave a difference of 71 French toises, or, rejecting the barometric measurements of Borda, of 18 toises.—*Humboldt, Pers. Nar.* v. 1, p. 160, 170. Saussure is said to have found water boil at 187° on the summit of Mont Blanc, being, according to Humboldt, 15,660. It is 90 feet only below the point on Pichincha, where I found it to boil at 186°. The elevations nearly equal the difference cannot amount to a degree; and I consider the error less likely to be on my side, because I was aware of the probable cause of error, and had to deduce the height from the accuracy of the observation. Humboldt in the same manner suspects the accuracy of Lamouroux's observation on the peak of Teneriffe.—*P. Nar.* vol. i. p. 159.

[To be continued.]

XXXIII. *A Letter to Professor Forbes on his communication on the colour of Steam in the Philosophical Magazine of Feb.* 1839. *By* Thomas Webster, M.A., *Sec. Inst. C. E.**

My Dear Sir,

ALLOW me to address to you, through the medium of the Philosophical Magazine, a few remarks on your most valuable observations on the colour of steam.

The conclusion to which you have been led, that the colours of steam by transmitted light are due to a particular stage of the condensing process, appears to me likely to furnish information on points with which we are at present totally unacquainted, and particularly with respect to the constitution of steam, and the conversion of sensible into latent caloric, when steam suddenly expands. We know that the hand may be held in high pressure steam issuing from an orifice, and that highly elastic steam allowed to expand into a partial vacuum will instantly resume its original or liquid form, which phænomena are perfectly consistent with the general law of the absorption of heat on the dilatation of bodies; but of the law of the diminution of temperature consequent on this absorption we are totally ignorant. If the sum of the latent and sensible heat be constant for steam of all elasticities, this con-

* Communicated by the Author.

version must be much more rapid for high than for low steam, and consequently the stages of condensation must be passed more rapidly in the former than in the latter case. I am not aware of the existence of any authentic observations on the temperature of highly elastic steam when expanded into a partial vacuum, or at different distances from the orifice, nor do I think that the thermometer will furnish the requisite knowledge, as its changes are slow, and the changes to be ascertained are exceedingly rapid, and unless noted at the instant the numerous sources of inaccuracy will embarrass the results. However, the thermometer will furnish no evidence respecting the caloric of elasticity, as it has been termed, or of the state of the particles in the progress of the steam towards condensation. If these successions of colour are due to the stage of condensation, they ought, on the preceding principles, to succeed each other more rapidly, and the critical stage ought to appear nearer the orifice the higher the elastic force of the steam. Thus I conceive your observations on the colour of steam may be employed as a test of the truth of the above principles, as a measure of the conversion of sensible into latent caloric when steam suddenly expands, as a means of obtaining some distinct and accurate knowledge of the elasticity and temperature of steam during its expansion—laws which cannot be ascertained by the mercurial column and the thermometer, owing to the time which their indications require, and finally of the various transition states through which the particles pass betwixt a colourless elastic and inelastic fluid.

Your experiments and observations, and the conclusions which may be derived from them, are the only ones on which I can rely with confidence, as supporting an opinion long entertained by me respecting the theory of clouds. I have never been able to assent to the theories generally circulated on this subject, but have considered clouds as masses of vapour still preserving its gaseous form, but in a different stage as regards condensation from the surrounding vapour, and that while their shape depends on the manner in which the transfer of heat is going on, their colour depends on the state of transition in which the particles of the mass exist, and on the position of the mass with respect to the illuminating body and the spectator. Thus their appearance will continue the same only so long as the above conditions are unaltered, and a cloud which appears stationary or in motion may in reality be a mass of vapour in motion or at rest.

Should these brief remarks be of any value in themselves, or turn the attention of any one to the important subject which

you have brought forward, I am sure you will excuse the liberty I have taken in thus addressing you.

Yours very truly,

25 Great George-street, Feb. 18, 1839. THOMAS WEBSTER.

XXXIV. *Some remarks on Hydrocyanic Acid. By* J. T. COOPER, Esq., *Lecturer on Chemistry, &c.**

IN the month of November 1831 I prepared some anhydrous hydrocyanic acid, by passing a current of dry sulphuretted hydrogen through a tube containing dry cyanide of mercury, and condensed the product by a freezing mixture surrounding the receivers; almost immediately after its preparation its specific gravity was taken at the temperature of 37° Fahr., which was found to be 0·706; and in two hours and a half after its preparation, I proceeded to determine its refractive index, by inclosing a portion of the acid in a perfectly air-tight hollow prism, whose refracting angle was 49° 15′ 40″, which, adopting the notation of Sir J. F. W. Herschel, we will call $\angle$ I; then by a very simple instrument which I am in the habit of using for these purposes, the angle D, or that made by the incident and refracted ray, was found to be for Frauenhofer's ray A 14° 41′, and for ray H 15° 13′ 30″. Hence by adopting Sir J. Herschel's formula, viz. $\mu = \dfrac{\sin\left(\dfrac{D}{2}+\dfrac{I}{2}\right)}{\sin\dfrac{I}{2}}$

in which expression μ represents the refractive index, we have

$$
\text{Temp. } 37^\circ \left\{
\begin{array}{lrl}
\text{for ray A} & \dfrac{D}{2} = & 7^\circ\ 20'\ 30'' \\
\text{extreme red} & \dfrac{I}{2} = & 24\ 37\ 50 \quad \text{log. sin. } 9{\cdot}61989 \\
 & \left(\dfrac{D}{2}+\dfrac{I}{2}\right) = & 31\ 58\ 20 \quad \text{log. sin. } 9{\cdot}72387 \\
 & \mu = & 1{\cdot}2705 \quad \text{log. } \quad {\cdot}10398 \\
\text{and for ray H} & \dfrac{D}{2} = & 7\ 36\ 45 \\
\text{extreme violet} & \dfrac{I}{2} = & 24\ 37\ 50 \quad \text{log. sin. } 9{\cdot}61989 \\
 & \left(\dfrac{D}{2}+\dfrac{I}{2}\right) = & 32\ 14\ 35 \quad \text{log. sin. } 9{\cdot}72714 \\
 & \mu = & 1{\cdot}2801 \quad \text{log. } \quad {\cdot}10725
\end{array}
\right.
$$

* Communicated by the Author, from whom it was received in the course of last month.—EDIT.

$$\begin{array}{ll} \mu \text{ for ray A} = 1{\cdot}2705 & \mu \text{ for ray H} = 1{\cdot}2801 \\ \mu \text{ for ray H} = \underline{1{\cdot}2801} & \mu \text{ for ray A} = \underline{1{\cdot}2705} \\ \qquad 2)\overline{2{\cdot}5506} & \qquad \delta\mu \quad = \ {\cdot}0096 \\ \qquad\quad \overline{1{\cdot}2753} & \end{array} \quad \therefore \frac{\delta\mu}{\mu - 1} =$$

$$= \frac{{\cdot}0096}{{\cdot}2753} = {\cdot}0035 = p \text{ its dispersive power.}$$

With a view of ascertaining whether any and what change the acid so prepared would undergo in the course of time, it was put into a stoppered phial, which had also a cap or cover of glass fitted over the stopper, so as the more effectually to secure the acid from evaporation, and the phial was then put into a tin box to exclude it from the action of light; and so *carefully* was this put aside, that I lost all trace of it until a few weeks since, when upon re-examination, I found, that to appearance, as far as I can remember the quantity the phial contained, it has suffered no diminution in bulk, nor was it perceptibly altered in appearance excepting a very minute deposit of a light grey matter, which I believe to be lead derived from the glass; and on retaking its specific gravity and its refractive index, there was found to be no appreciable difference from the results formerly obtained in either.

The above facts go to prove that real hydrocyanic acid, prepared by the above process, is capable of being preserved for a length of time in close vessels if light be excluded; the same portion of acid is now, and has been for more than a fortnight, exposed to diffuse daylight, and as yet has shown no signs of decomposition; indeed, having frequently for the purpose of demonstration had occasion to prepare the anhydrous acid, I have remarked, contrary to the general opinion, that the acid so prepared, under ordinary circumstances of keeping, that is, of exposure to common daylight, that not one portion in five or six undergoes any change by depositing the peculiar brown matter, if the phials are perfectly well stopped. If the stability of this acid should ultimately turn out to be different from what is generally imagined, it is not improbable that in consequence of its possessing so low a refractive index and dispersive power, that it may be made available for some useful optical purposes.

Now it has been stated by Dr. Brewster, that the refractive index of cyanogen liquefied by pressure is 1·316; hence it is possible, I conceive, to deduce, without the risk of incurring any very considerable error, what the refractive index of hydrogen would be if it were possible to obtain that body in a liquid state; for as hydrogen and cyanogen combine in equal volumes to form hydrocyanic acid, and their union occurs in

the gaseous state without any condensation of volume, it may be presumed that if we subtract the refractive index of hydrocyanic acid from that of cyanogen, viz. ·275 from ·316 = ·041 for the refractive index of liquid hydrogen.

82 Blackfriars Road, London, Feb. 1, 1836.

XXXV. *On the Motion and Rest of rigid Bodies.* By J. J. Sylvester, *Professor of Natural Philosophy in University College, London.**

IN the subjoined investigation, which, as far as I know, is my own, I apply the same method to rigid as in a preceding paper I applied to fluid systems.

Let $x\ y\ z$ be the coordinates of any particle in a rigid body;

$x'\ y'\ z'$ the coordinates of some other particle, and let $\quad x' = x + h \qquad y' = y + k \qquad z' = z + l.$

Call Δx, Δy, Δz the increments which x, y, z receive after the lapse of a small interval of time; so that terms in which they enter in two or more dimensions may be neglected.

$$\text{Then } \Delta(x') = \Delta x + \frac{d\Delta x}{dx} h + \frac{d\Delta x}{dy} k + \frac{dx}{dz} . l + \text{P}$$

$$\Delta(y') = \Delta y + \frac{d\Delta y}{dx} . h + \frac{d\Delta y}{dy} . k + \frac{d\Delta y}{dz} l + \text{Q}$$

$$\Delta(z') = \Delta z + \frac{d\Delta z}{dx} h + \frac{d\Delta z}{dy} . k + \frac{d\Delta z}{dz} . l + \text{R}.$$

P, Q, R containing binary and higher combinations of h, k, l, which we shall have no occasion to express.

At the commencement of the interval the squared distance of the two particles was $(x'-x)^2 + (y'-y)^2 + (z'-z)^2$; at the end of the interval the distance squared is

$$(x'-x + \Delta('x) - \Delta x)^2 + (y'-y + \Delta(y') - \Delta y)^2 + (z'-z + \Delta(z') - \Delta z)^2.$$

and these two expressions must be the same by the conditions of rigidity whatever h, k, and l may be; i. e.

$$h^2+k^2+l^2 = \left(h + \frac{d\Delta x}{dx} h + \frac{d\Delta y}{dy} k + \frac{d\Delta z}{dz} . l + \text{P}\right)^2$$

$$+ \left(k + \frac{d\Delta y}{dx} h + \frac{d\Delta y}{dy} k + \frac{d\Delta y}{dz} l + \text{Q}\right)^2$$

$$+ \left(l + \frac{d\Delta z}{dx} h + \frac{d\Delta z}{dy} k + \frac{d\Delta z}{dz} . l + R\right)^2$$

for all values of h, k, and l.

* Communicated by the Author.

Hence rejecting infinitesimals of the second order and equating to zero separately the coefficients of h^2, k^2, l^2, and of $k\,l$, $l\,k$, $h\,k$, we have

$$\frac{d\,\Delta x}{d\,x} = 0 \ (a) \quad \frac{d\,\Delta y}{d\,z} + \frac{d\,\Delta z}{d\,y} = 0 \qquad (d.)$$

$$\frac{d\,\Delta y}{d\,y} = 0 \ (b) \quad \frac{d\,\Delta z}{d\,x} + \frac{d\,\Delta x}{d\,z} = 0 \qquad (e.)$$

$$\frac{d\,\Delta z}{d\,z} = 0 \ (c) \quad \frac{d\,\Delta x}{d\,y} + \frac{d\,\Delta y}{d\,x} = 0 \qquad (f.)$$

By differentiating (d), (e), (f) with respect to z, x, y respectively, and substituting from (a), (b), (c), we obtain

$$\frac{d^2\,\Delta y}{d\,z^2} = 0 \quad \frac{d^2\,\Delta z}{d\,x^2} = 0 \quad \frac{d^2\,\Delta x}{d\,y^2} = 0.$$

By differentiating the same with respect to y, z, x respectively, and proceeding as before, we have

$$\frac{d^2\,\Delta z}{d\,y^2} = 0 \quad \frac{d^2\,\Delta x}{d\,z^2} = 0 \quad \frac{d^2\,\Delta z}{d\,x^2} = 0.$$

Thus, then, we have

$$\frac{d\,\Delta x}{d\,x} = 0 \quad \frac{d^2\,\Delta x}{d\,y^2} = 0 \quad \frac{d^2\,\Delta x}{d\,z^2} = 0$$

$$\frac{d\,\Delta y}{d\,y} = 0 \quad \frac{d^2\,\Delta y}{d\,z^2} = 0 \quad \frac{d^2\,\Delta y}{d\,x^2} = 0$$

$$\frac{d\,\Delta z}{d\,z} = 0 \quad \frac{d^2\,\Delta z}{d\,x^2} = 0 \quad \frac{d^2\,\Delta z}{d\,y^2} = 0$$

$$\therefore \ \Delta x = \mathrm{A} + \mathrm{B}\,y + \mathrm{C}\,z \qquad (o.)$$

$$\Delta y = \mathrm{D} + \mathrm{E}\,z + \mathrm{F}\,x \qquad (p.)$$

$$\Delta z = \mathrm{G} + \mathrm{H}\,x + \mathrm{K}\,y \qquad (q.)$$

A, B, C, D, E, F, being constant for a *given instant* of time; between which by virtue of the $=^{\text{ns}}$ (d), (e), (f), we have the relations

$$\mathrm{E} + \mathrm{K} = 0 \qquad \mathrm{H} + \mathrm{C} = 0 \qquad \mathrm{B} + \mathrm{F} = 0$$

If we call u, v, w the three component velocities of the particles at x, y, z parallel to the three axes, and X_i Y_i Z_i the three internal forces, it is at once seen that u, v, w, as also $\Delta\,X_i$ Y_i $\Delta_i\,Z_i$ must be subject to the same equations as limit $\Delta\,x$, $\Delta\,y$, $\Delta\,z$;

so that
$$u = a + \gamma y - \beta z \ (1) \quad \Delta\,X_i = a_i + \gamma_i y - \beta_i z \quad (h.)$$
$$v = b + \alpha z - \gamma x \ (2) \quad \Delta\,Y_i = b_i + \alpha_i z - \gamma x_i \quad (f.)$$
$$w = c + \beta x - \alpha y \ (3) \quad \Delta\,Z_i = c_i + \beta_i x - \alpha_i y \quad (k.)$$

Also if $X_, Y_, Z$ be the impressed forces, we have

$$X_, + X = \frac{du}{dt} \qquad (4)$$

$$Y_, + Y = \frac{dv}{dt} \qquad (5)$$

$$Z_, + Z = \frac{dw}{dt} \qquad (6).$$

And by Gauss's principle, calling m the mass of the particle at $x_, y_, z_,$ $\Delta \Sigma m . \{\overline{X_,^2 + Y_,^2 - Z_,^2}\} = 0.$

Hence equating separately to zero the coefficients of $a_, b_, c_,$ and of $\alpha_, \beta_, \gamma_,$ in the quantity $\Sigma m (X_, \Delta X_, + Y_, \Delta y + Z_, \Delta Z_,)$ we have

$$\left.\begin{array}{l} \Sigma m . X_, = 0 \\ \Sigma m . Y_, = 0 \\ \Sigma m . Z_, = 0 \\ \Sigma m \ (Z_, y - Y_, z) = 0 \\ \Sigma m . (Y_, x - X_, y) = 0 \\ \Sigma m . (X_, z - Z_, x) = 0 \end{array}\right\} \ 7-12$$

Lastly, we have the $=$ ns

$$u = \frac{dx}{dt} \quad (13)$$

$$v = \frac{dy}{dt} \quad (14)$$

$$w = \frac{dz}{dt} \quad (15)$$

From the fifteen equations marked 1 to 15, the motion may be determined by assigning the position of each particle at the end of the time t in terms of its three initial coordinates, its three initial velocities, and the initial values of the nine quantities.

$$\begin{array}{lll} \Sigma m . x & \Sigma m y z & \Sigma m x^2 \\ \Sigma m . y & \Sigma m z x & \Sigma m y^2 \\ \Sigma m . z & \Sigma m x y & \Sigma m z^2 \end{array}$$

In the case of rest $X_, = -X \quad Y_, = -Y \quad Z_, = -Z$, and the $=$ ns 7 to 12 inclusively taken, express the conditions of equilibrium.

The equations o, p, q, which have been obtained from conditions *purely geometrical*, establish the well-known but interesting and *not obvious* fact, that any *small* motion of a rigid body may be conceived as made up of a motion of translation and a motion about *one* axis.

University College, London, Dec. 8, 1838. J. J. Sylvester.

XXXVI. *On the Colours of Mixed Plates.* *By* Sir David Brewster, *K.G.H., F.R.S.**

THE colours of mixed plates were discovered by Dr. Thomas Young†, and described in the Philosophical Transactions for 1802. He produced them by interposing small portions of water, or butter, or tallow between two plates of glass, or two object glasses pressed together so as to give the ordinary colours of thin plates. In this way portions or cavities of air were surrounded with water, butter, or tallow; and on looking through this combination of media he saw fringes or rings of colour six times larger than those of thin plates that would have been produced had air alone been interposed between the glasses. These fringes or rings of colour were seen by the direct light of a candle, and began from a white centre like those produced by transmission; but on the dark space next the edge of the plate, Dr. Young observed another set of fringes or rings, complementary to the first, and beginning from a black centre like those produced by reflection. This last set of colours was always brighter than the first.

The following is Dr. Young's explanation of these two series of colours.

"In order to understand," says he, "this circumstance, we must consider that where a dark object is placed behind the glasses, the whole of the light which comes to the eye is either refracted through the edges of the drops, or reflected from the internal surface; while the light which passes through those parts which are on the side opposite to the dark object consists of rays refracted as before through the edges, or simply passing through the fluid. The respective combinations of these portions of light exhibit a series of colours of different orders, since the internal reflection modifies the interference of the rays on the dark side of the object, in the same manner as in the common colours of thin plates seen by reflection. When no dark object is near, both these series of colours are produced at once; and since they are always of an opposite nature at any given thickness of a plate, they neutralize each other and constitute white light ‡."

* From the Philosophical Transactions for 1838, p. 73.

† Since this paper was written I find that this class of colours was discovered by M. Mazeas, and that his experiments were repeated and varied by M. Dutour.

‡ Philosophical Transactions, 1802. Dr. Young republished the same explanation of mixed plates in 1807 in his Elements of Natural Philosophy. See vol. i. pp. 470, 787; vol. ii. pp. 635, 680.

In so far as I know, these observations have not been repeated by any other philosopher; and subsequent authors have only copied Dr. Young's description of the phænomena and acquiesced in his explanation of them. In taking up this subject I never doubted the accuracy or the generality of the results obtained by so distinguished a philosopher. I was induced to study the phænomena of mixed plates as auxiliary to a more general inquiry; and having observed new phænomena of colour in mineral bodies, which have the same origin as those of mixed plates, and which lead to conclusions different from those of Dr. Young, I am anxious that they should be described in the same work which contains his original observations.

Having experienced considerable difficulty in obtaining satisfactory specimens of the colours of mixed plates by using the substances employed by Dr. Young, I sought for a method of producing them which should be at once easy and infallible in its effects. With this view I tried transparent soap, and whipped cream, which gave tolerably good results; but I obtained the best effects by using the white of an egg beat up into froth. To obtain a proper film of this substance I place a small quantity between the two glasses, and having pressed it out into a film I separate the glasses, and by holding them near the fire I drive off a little of the superfluous moisture. The two glasses are again placed in contact, and when pressed together so as to produce the coloured fringes or rings, they are then kept in their place either by screws or by wax, and may be preserved for any length of time.

If we now examine with a magnifier of small power the thin film of albumen, we shall find that it contains thousands of cavities exactly resembling the strata of cavities which I have described as occurring in topaz, quartz, sulphate of lime and other minerals*; and if we look through the film at the margin of the flame of a candle, we shall perceive the two sets of colours described by Dr. Young, the one upon the luminous edge of the flame, and the other on the dark space contiguous to it. The first we shall call the *direct*, and the second, which are always the brightest, the *complementary fringes.*

If we apply a higher magnifying power to the albuminous films, and bring the edge of one of the cavities to the margin of the flame, we shall perceive that both the *direct* and the *complementary* colours are formed at the very edge, the complementary ones appearing just when the direct ones have disappeared, by the withdrawal of the edge from the flame.

* Edin. Trans., vol. x. Part I. p. 407.

As the colours therefore are produced solely by the edges of the cavities, their intensity must, *cæteris paribus*, depend on the smallness of the cavities, or the number of edges which occur in a given space. When we succeed in forming an uniform film in which the cavities are like a number of minute points, the phænomena are peculiarly splendid and we are enabled to study them with greater facility. When the edges of these cavities are seen by an achromatic microscope, and in direct light, neither the direct nor the complementary colours are visible; but if we gradually withdraw the lens from the cavities a series of beautiful phænomena appear. When the vision first becomes indistinct both the direct and the complementary colours appear at the same time, specks of the *complementary red* alternating with brighter specks of the *direct green* light. By increasing the distance of the lens from the cavities, the complementary specks become less and less visible, and we see only the direct green light.

In order to study these phænomena by observing the action of a *single* edge upon light, and to ascertain the effect of an edge when there were no prismatic edges to refract, and no internal surface to reflect light, I conceived the idea of immersing thin plates of a solid substance in a fluid of such a refractive power, that the thickness of the plates should be virtually reduced to the same degree of thinness as the film of albumen between the plates of glass. The new substance described by Mr. Horner*, and which I shall call *nacrite*, furnished me with the means of performing this experiment. I accordingly inclosed the thinnest films of it between two plates of glass containing balsam of capivi; and I had the satisfaction of observing that the bounding edge of the plate and the fluid produced the identical direct and complementary colours above described.

The bounding edge which I selected for observation gave a *bright green* for the *direct*, and a *bright red* for the *complementary* tint. This edge appeared as a narrow distinct black line, exceedingly well defined, and of a uniform breadth like the finest micrometer wire. It consequently obstructed the incident light and produced the phænomena of diffracted fringes. These fringes, however, were modified by the peculiar circumstances under which they were produced, and exhibited in their tints both the direct and complementary colours under consideration.

When the diffracted fringes are viewed in candle-light by a

* Philosophical Transactions, 1836, p. 49. [or L. and E. Phil. Mag., vol. x. p. 201.

lens placed at a greater distance from the diffracting edge than its principal focus, the middle of the system of fringes corresponding to the diffracted shadow of a fibre is occupied with the *direct tint*, which we shall suppose to be *green*; and on each side of this *green* shadow, as we may call it, we observe very faintly the *complementary red* tinging what are called the two first exterior fringes. This tinge of red is strongest in the first fringe within the solid edge, or within the green shadow, while it is *yellowish* in the first fringe without the green shadow. These effects are inverted if we place the lens nearer to the edge than its principal focus.

The phænomena now described appear more distinct if we take an extremely narrow piece of nacrite, having its two edges nearly in contact, and transmitting only a narrow line of light. In this case the two red fringes within the solid edge unite their tints, and become a bright red; and in like manner if we place the lens nearer the solid edges than its principal focus, the two yellow fringes will unite their tints, and become a brighter yellow band. In this last case, when the two bounding edges are still nearer each other, the united fringes, in place of being yellow, will be *green*, or the same as the direct colour.

If we bring the edges of two pieces of nacrite of equal thickness very near each other, having, as formerly, *green* for the *direct*, and *red* for the *complementary* colour, the space between the edges, or between the green bands, will be faint *red* when the lens is nearer the edges than its principal focus, and *yellow* when it is further from them; but if the edges are brought still nearer, the faint red will become brighter, and the united green bands will take the place of the yellow one.

Let us now return to our plate of *nacrite* with a single edge, having *green* and *red* for the two tints: and let us always suppose that the lens is adjusted to observe the diffracted fringes, that is, that the lens is placed at a greater distance from the diffracting edge than its principal focus. We shall also suppose that the light of the sun passing through a narrow aperture parallel to the diffracting edge is substituted for the light of a candle. Under these circumstances the central part of the system of fringes seen by light incident perpendicularly, consists of *blue**, *green*, and *yellow* light, constituting, as it were, the shadow of the edge, the blue light being on the same side as the plate of nacrite, and the yellow rays encroaching upon the exterior faint red band already described, the other red band next the blue being more distinctly seen. If

* Owing to the small quantity of blue rays in candle-light the blue almost disappears in it.

we now incline the incident ray to the plate of nacrite more than 90°, the faint red band next the yellow gradually becomes brighter, while the other bands become fainter; and at the boundary of light and darkness all the other bands disappear except this *red* one, which is the *complementary* colour to the *green*, (produced by the union of the *blue*, *green* and *yellow* bands,) and the colour which is seen upon the dark space next the edge of the flame, as described by Dr. Young. If we, on the other hand, incline the incident ray in an opposite direction, so that it forms with the plane of the plate a less angle than 90°, the *red* band next the blue will now become brighter; and at the boundary of light and darkness, when all the other bands have disappeared, the *red* band will afford the complementary colour to the *green*.

As the edge of the plate of nacrite is rough and unpolished, and accurately perpendicular to the parallel faces, there are no reflected nor refracted pencils, whose combinations with one another, or with the direct rays, can be employed to account for the complementary colours. The phænomena of mixed plates, indeed, are cases of diffraction when the light is obstructed by the edge of very thin transparent plates placed in a medium of different refractive power. If the plate were opake, the fringes would be exactly those which have been so often described, and explained by the principle of interference. But owing to the *transparency* of the plate, fringes are produced within its shadow; and owing to the *thinness* of the plate the light transmitted through it and retarded, interferes with the partial waves which pass through the plate and with those which pass beyond the diffracting edge with undiminished velocity, and modifies the usual system of fringes in the manner which we have described.

As the plate of nacrite diminishes in thickness, or as the fluid in which it is immersed approaches to it in refractive density, the central coloured bands, whose union constitutes the *direct* tint, will diminish in number, and descending gradually in the scale will finally disappear when the retardation produced by the plate does not perceptibly alter the phase of the ray. When the plate, on the other hand, increases in thickness, or the fluid diminishes in refractive power, the central bands will become closer and more numerous, and will finally resemble the fringes within the shadow of the ordinary system.

When the plate of nacrite is thicker at one place than another by the partial removal of a parallel film, the edge where the increase of thickness takes place produces exactly the same phænomena as the edge of the film that is removed, or of the film that is elevated above the general surface, and hence we

are led to look for the phænomena of mixed plates in minerals, such as *sulphate of lime* and *mica*, where a plate of two different thicknesses can be easily obtained. I have accordingly discovered the phænomena of mixed plates distinctly exhibited in sulphate of lime and mica.

A more splendid exhibition of these colours is seen when a stratum of cavities of extreme thinness occurs in sulphate of lime. I have observed such strata repeatedly in the gypsum from Mont-martre; but they are most beautiful when the stratum has a circular form. In this case the cavities are exceedingly thin at the circumference of the circle, and gradually increase in depth towards the centre, so that we have a series of edges increasing in thickness towards a centre; the very reverse of a mixed plate, such as a film of albumen pressed between two convex surfaces. The system of rings is therefore also reversed, the highest order of colours being in the centre, while the lowest are at the circumference of the circular stratum. In many strata of cavities, such as the one which I have engraven in my paper on the new fluids in minerals*, the cavities are too deep to give the colours of mixed plates.

Another example of the colours of mixed plates in natural bodies occurs in specimens of mica, through which titanium is disseminated in beautiful flat dendritic crystals of various degrees of opacity and transparency. In these specimens the titanium is often disseminated in grains, forming an irregular surface. The edges of these grains, by retarding the light which they transmit, produce the direct and complementary colours of mixed plates in the most perfect manner, the tints passing through two orders of colours as the grains of titanium increase in size towards the interior of the irregular patch. I have observed another example of these colours in the deep cavities of topaz, from which the fluids have either escaped, leaving one or both of the surfaces covered with minute particles of transparent matter, or in which the fluids have suffered induration.

Allerly by Melrose, Oct. 18, 1837.

XXXVII. *Some Account of the Art of Photogenic Drawing.* *By* H. F. Talbot, *Esq.*, *F.R.S.*†

§ 1.

IN the spring of 1834 I began to put in practice a method which I had devised some time previously, for employing

* Edinburgh Transactions, vol. x. plate ii. fig. 33.

† Read before the Royal Society on the 31st of January, and communicated by the Author.

to purposes of utility the very curious property which has been long known to chemists to be possessed by the nitrate of silver; namely, its discoloration when exposed to the violet rays of light. This property appeared to me to be perhaps capable of useful application in the following manner.

I proposed to spread on a sheet of paper a sufficient quantity of the nitrate of silver, and then to set the paper in the sunshine, having first placed before it some object casting a well-defined shadow. The light, acting on the rest of the paper, would naturally blacken it, while the parts in shadow would retain their whiteness. Thus I expected that a kind of image or picture would be produced, resembling to a certain degree the object from which it was derived. I expected, however, also, that it would be necessary to preserve such images in a portfolio, and to view them only by candle-light; because if by daylight, the same natural process which formed the images would destroy them, by blackening the rest of the paper.

Such was my leading idea before it was enlarged and corrected by experience. It was not until some time after, and when I was in possession of several novel and curious results, that I thought of inquiring whether this process had been ever proposed or attempted before? I found that in fact it had; but apparently not followed up to any extent, or with much perseverance. The few notices that I have been able to meet with are vague and unsatisfactory; merely stating that such a method exists of obtaining the outline of an object, but going into no details respecting the best and most advantageous manner of proceeding.

The only definite account of the matter which I have been able to meet with, is contained in the first volume of the Journal of the Royal Institution, page 170, from which it appears that the idea was originally started by Mr. Wedgwood, and a numerous series of experiments made both by him and Sir Humphry Davy, which however ended in failure. I will take the liberty of quoting a few passages from this memoir.

"The copy of a painting, immediately after being taken, must be kept in an obscure place. It may indeed be examined in the shade, but in this case the exposure should be only for a few minutes. No attempts that have been made to prevent the uncoloured parts from being acted upon by light, have as yet been successful. They have been covered with a thin coating of fine varnish; but this has not destroyed their susceptibility of becoming coloured. When the solar rays are passed through a print and thrown upon prepared paper,

the unshaded parts are slowly copied; but the lights transmitted by the shaded parts are seldom so definite as to form a distinct resemblance of them by producing different intensities of colour.

"The images formed by means of a *camera obscura* have been found to be too faint to produce, in any moderate time, an effect upon the nitrate of silver. To copy these images was the first object of Mr. Wedgwood, but all his numerous experiments proved unsuccessful."

These are the observations of Sir Humphry Davy. I have been informed by a scientific friend that this unfavourable result of Mr. Wedgwood's and Sir Humphry Davy's experiments, was the chief cause which discouraged him from following up with perseverance the idea which he had also entertained of fixing the beautiful images of the *camera obscura*. And no doubt, when so distinguished an experimenter as Sir Humphry Davy announced "that all experiments had proved unsuccessful," such a statement was calculated materially to discourage further inquiry. The circumstance also, announced by Davy, that the paper on which these images were depicted was liable to become entirely dark, and that nothing hitherto tried would prevent it, would perhaps have induced me to consider the attempt as hopeless, if I had not (fortunately) before I read it, already discovered a method of overcoming this difficulty, and of *fixing* the image in such a manner that it is no more liable to injury or destruction.

In the course of my experiments directed to that end, I have been astonished at the variety of effects which I have found produced by a very limited number of different processes when combined in various ways; and also at the length of time which sometimes elapses before the full effect of these manifests itself with certainty. For I have found that images formed in this manner, which have appeared in good preservation at the end of twelve months from the time of their formation, have nevertheless somewhat altered during the second year. This circumstance, added to the fact that the first attempts which I made became indistinct in process of time (the paper growing wholly dark), induced me to watch the progress of the change during some considerable time, as I thought that perhaps *all* these images would *ultimately* be found to fade away. I found, however, to my satisfaction, that this was not the case; and having now kept a number of these drawings during nearly five years without their suffering any deterioration, I think myself authorized to draw conclusions from my experiments with more certainty.

§ 2. *Effect and Appearance of these Images.*

The images obtained in this manner are themselves white, but the ground upon which they display themselves is variously and pleasingly coloured.

Such is the variety of which the process is capable, that by merely varying the proportions and some trifling details of manipulation, any of the following colours are readily attainable:

Sky-blue,	Brown, of various shades,
Yellow,	Black.
Rose-colour,	

Green alone is absent from the list, with the exception of a dark shade of it, approaching to black. The blue-coloured variety has a very pleasing effect, somewhat like that produced by the Wedgwood-ware, which has white figures on a blue ground. This variety also retains its colours perfectly if preserved in a portfolio, and not being subject to any spontaneous change, requires no preserving process.

These different shades of colour are of course so many different chemical compounds, or mixtures of such, which chemists have not hitherto distinctly noticed.

§ 3. *First Applications of this Process.*

The first kind of objects which I attempted to copy by this process were flowers and leaves, either fresh or selected from my herbarium. These it renders with the utmost truth and fidelity, exhibiting even the venation of the leaves, the minute hairs that clothe the plant, &c.

It is so natural to associate the idea of *labour* with great complexity and elaborate detail of execution, that one is more struck at seeing the thousand florets of an *Agrostis* depicted with all its capillary branchlets (and so accurately, that none of all this multitude shall want its little bivalve calyx, requiring to be examined through a lens), than one is by the picture of the large and simple leaf of an oak or a chestnut. But in truth the difficulty is in both cases the same. The one of these takes no more time to execute than the other; for the object which would take the most skilful artist days or weeks of labour to trace or to copy, is effected by the boundless powers of natural chemistry in the space of a few seconds.

To give an idea of the degree of accuracy with which some objects can be imitated by this process, I need only mention one instance. Upon one occasion, having made an image of a piece of lace of an elaborate pattern, I showed it to some persons at the distance of a few feet, with the inquiry, whether it

was a good representation? when the reply was, "That they were not to be so easily deceived, for that it was evidently no picture, but the piece of lace itself."

At the very commencement of my experiments upon this subject, when I saw how beautiful were the images which were thus produced by the action of light, I regretted the more that they were destined to have such a brief existence, and I resolved to attempt to find out, if possible, some method of preventing this, or retarding it as much as possible. The following considerations led me to conceive the possibility of discovering a preservative process.

The nitrate of silver, which has become black by the action of light, is no longer the same chemical substance that it was before. Consequently, if a picture produced by solar light is subjected afterwards to any chemical process, the white and dark parts of it will be differently acted upon; and there is no evidence that after this action has taken place, these white and dark parts will any longer be subject to a spontaneous change; or, if they are so, still it does not follow that that change will *now* tend to assimilate them to each other. In case of their remaining *dissimilar*, the picture will remain visible, and therefore our object will be accomplished.

If it should be asserted that exposure to sunshine would *necessarily* reduce the whole to one uniform tint, and destroy the picture, the *onus probandi* evidently lies on those who make the assertion. If we designate by the letter A the exposure to the solar light, and by B some indeterminate chemical process, my argument was this: Since it cannot be shown, *à priori*, that the final result of the series of processes A B A will be the same with that denoted by B A, it will therefore be worth while to put the matter to the test of experiment, viz. by varying the process B until the right one be discovered, or until so many trials have been made as to preclude all reasonable hope of its existence.

My first trials were unsuccessful, as indeed I expected; but after some time I discovered a method which answers perfectly, and shortly afterwards another. On one of these more especially I have made numerous experiments; the other I have comparatively little used, because it appears to require more nicety in the management. It is, however, equal, if not superior, to the first in brilliancy of effect.

This chemical change, which I call the *preserving process*, is far more effectual than could have been anticipated. The paper, which had previously been so sensitive to light, becomes completely insensible to it, insomuch that I am able to show the Society specimens which have been exposed for an hour

to the full summer sun, and from which exposure the image has suffered nothing, but retains its perfect whiteness.

§ 4. *On the Art of fixing a Shadow.*

The phænomenon which I have now briefly mentioned appears to me to partake of the character of the *marvellous*, almost as much as any fact which physical investigation has yet brought to our knowledge. The most transitory of things, a shadow, the proverbial emblem of all that is fleeting and momentary, may be fettered by the spells of our "*natural magic*," and may be fixed for ever in the position which it seemed only destined for a single instant to occupy.

This remarkable phænomenon, of whatever value it may turn out in its application to the arts, will at least be accepted as a new proof of the value of the inductive methods of modern science, which by noticing the occurrence of unusual circumstances (which accident perhaps first manifests in some small degree), and by following them up with experiments, and varying the conditions of these until the true law of nature which they express is apprehended, conducts us at length to consequences altogether unexpected, remote from usual experience, and contrary to almost universal belief. Such is the fact, that we may receive on paper the fleeting shadow, arrest it there, and in the space of a single minute fix it there so firmly as to be no more capable of change, even if thrown back into the sunbeam from which it derived its origin.

§ 5.

Before going further, I may however add, that it is not always necessary to use a preserving process. This I did not discover until after I had acquired considerable practice in this art, having supposed at first that all these pictures would ultimately become indistinct if not preserved in some way from the change. But experience has shown to me that there are at least two or three different ways in which the process may be conducted, so that the images shall possess a character of durability, provided they are kept from the action of direct sunshine. These ways have presented themselves to notice rather accidentally than otherwise; in some instances without any particular memoranda having been made at the time, so that I am not yet prepared to state accurately on what particular thing this sort of semi-durability depends, or what course is best to be followed in order to obtain it. But as I have found that certain of the images which have been subjected to no preserving process remain quite white and perfect after the lapse of a year or two, and indeed show no symptom whatever

of changing, while others differently prepared (and left unpreserved) have grown quite dark in one tenth of that time, I think this singularity requires to be pointed out. Whether it will be of much value I do not know; perhaps it will be thought better to incur at first the small additional trouble of employing the preserving process, especially as the drawings thus prepared will stand the sunshine; while the unpreserved ones, however well they last in a portfolio or in common daylight, should not be risked in a very strong light, as they would be liable to change thereby, even years after their original formation. This very quality, however, admits of useful application. For this semi-durable paper, which retains its whiteness for years in the shade, and yet suffers a change whenever exposed to the solar light, is evidently well suited to the use of a naturalist travelling in a distant country, who may wish to keep some memorial of the plants he finds, without having the trouble of drying them and carrying them about with him. He would only have to take a sheet of this paper, throw the image upon it, and replace it in his portfolio. The defect of this particular paper is, that in general the *ground* is not even; but this is of no consequence where utility alone, and not beauty of effect is consulted.

§ 6. *Portraits.*

Another purpose for which I think my method will be found very convenient, is the making of outline portraits, or *silhouettes.* These are now often traced by the hand from shadows projected by a candle. But the hand is liable to err from the true outline, and a very small deviation causes a notable diminution in the resemblance. I believe this manual process cannot be compared with the truth and fidelity with which the portrait is given by means of solar light.

§ 7. *Paintings on Glass.*

The shadow-pictures which are formed by exposing paintings on glass to solar light are very pleasing. The glass itself, around the painting, should be blackened; such, for instance, as are often employed for the magic lantern. The paintings on the glass should have no bright yellows or reds, for these stop the violet rays of light, which are the only effective ones. The pictures thus formed resemble the productions of the artist's pencil more, perhaps, than any of the others. Persons to whom I have shown them have generally mistaken them for such, at the same time observing, that the *style* was new to them, and must be one rather difficult to acquire. It is in these pic-

tures only that, as yet, I have observed indications of *colour*. I have not had time to pursue this branch of the inquiry further. It would be a great thing if by any means we could accomplish the delineation of objects in their natural colours. I am not very sanguine respecting the possibility of this; yet, as I have just now remarked, it appears possible to obtain at least *some indication* of variety of tint.

§ 8. *Application to the Microscope.*

I now come to a branch of the subject which appears to me very important and likely to prove extensively useful, the application of my method of delineating objects to the solar microscope.

The objects which the microscope unfolds to our view, curious and wonderful as they are, are often singularly complicated. The eye, indeed, may comprehend the whole which is presented to it in the field of view; but the powers of the pencil fail to express these minutiæ of nature in their innumerable details. What artist could have skill or patience enough to copy them? or granting that he could do so, must it not be at the expense of much most valuable time, which might be more usefully employed?

Contemplating the beautiful picture which the solar microscope produces, the thought struck me, whether it might not be possible to cause that image to impress itself upon the paper, and thus to let Nature substitute her own inimitable pencil, for the imperfect, tedious, and almost hopeless attempt of copying a subject so intricate.

My first attempt had no success. Although I chose a bright day, and formed a good image of my object upon prepared paper, on returning at the expiration of an hour I found that no effect had taken place. I was therefore half inclined to abandon this experiment, when it occurred to me, that there was no reason to suppose that either the nitrate or muriate of silver, as commonly obtained, was the most sensitive substance that exists to the action of the chemical rays*; and though such should eventually prove to be the fact, at any rate it was not to be assumed without proof. I therefore began a course of experiments in order to ascertain the influence of various modes of preparation, and I found these to be signally different in their results. I considered this matter chiefly in a practical point of view; for as to the theory, I confess that I cannot as yet understand the reason why the paper prepared in one way should be so much more sensitive than in another.

* Sir H. Davy somewhere says that the iodide is more sensitive, which I have hardly found to be the case in my experiments.

The result of these experiments was the discovery of a mode of preparation greatly superior in sensibility to what I had originally employed: and by means of this, all those effects which I had before only anticipated as theoretically possible were found to be capable of realization.

When a sheet of this, which I shall call "*Sensitive Paper*," is placed in a dark chamber, and the magnified image of some object thrown on it by the solar microscope, after the lapse of perhaps a quarter of an hour, the picture is found to be completed. I have not as yet used high magnifying powers, on account of the consequent enfeeblement of the light. Of course, with a more sensitive paper, greater magnifying power will become desirable.

On examining one of these pictures, which I made about three years and a half ago, I find, by actual measurement of the picture and the object, that the latter is magnified seventeen times in linear diameter, and in surface consequently 289 times. I have others which I believe are considerably more magnified; but I have lost the corresponding objects, so that I cannot here state the exact numbers.

Not only does this process save our time and trouble, but there are many objects, especially microscopic crystallizations, which alter so greatly in the course of three or four days (and it could hardly take any artist less to delineate them in all their details), that they could never be drawn in the usual way.

I will now describe the *degree of sensitiveness* which this paper possesses, premising that I am far from supposing that I have reached the limit of which this quality is capable. On the contrary, considering the few experiments which I have made, (few, that is, in comparison with the number which it would be easy to imagine and propose) I think it most likely, that other methods may be found, by which substances may be prepared, perhaps as much transcending in sensitiveness the one which I have employed, as that does the nitrate of silver which I used in my first experiments.

But to confine myself to what I have actually accomplished, in the preparation of a very sensitive paper. When a sheet of this paper is brought towards a window, not one through which the sun shines, but looking in the opposite direction, it immediately begins to discolour. For this reason, if the paper is prepared by daylight, it must by no means be left uncovered, but as soon as finished be shut up in a drawer or cupboard and there left to dry, or else dried at night by the warmth of a fire. Before using this paper for the delineation of any object, I generally approach it for a little time towards the light, thus intentionally giving it a slight shade of colour, for the

purpose of seeing that the *ground* is *even.* If it appears so when thus tried to a small extent, it will generally be found to prove so in the final result. But if there are some places or spots in it which do not acquire the same tint as the rest, such a sheet of paper should be rejected: for there is a risk that, when employed, instead of presenting a *ground* uniformly dark, which is essential to the beauty of the drawing, it will have large white spots, places altogether insensible to the effect of light. This singular circumstance I shall revert to elsewhere: it is sufficient to mention it here.

The paper then, which is thus readily sensitive to the light of a common window, is of course much more so to the direct sunshine. Indeed, such is the velocity of the effect then produced, that the picture may be said to be ended almost as soon as it is begun.

To give some more definite idea of the rapidity of the process, I will state, that after various trials the nearest evaluation which I could make of the time necessary for obtaining the picture of an object, so as to have pretty distinct outlines, when I employed the full sunshine, was *half a second.*

§ 9. *Architecture, Landscape, and external Nature.*

But perhaps the most curious application of this art is the one I am now about to relate. At least it is that which has appeared the most surprising to those who have examined my collection of pictures formed by solar light.

Every one is acquainted with the beautiful effects which are produced by a camera obscura and has admired the vivid picture of external nature which it displays. It had often occurred to me, that if it were possible to retain upon the paper the lovely scene which thus illuminates it for a moment, or if we could but fix the outline of it, the lights and shadows, divested of all *colour*, such a result could not fail to be most interesting. And however much I might be disposed at first to treat this notion as a scientific dream, yet when I had succeeded in fixing the images of the solar microscope by means of a peculiarly sensitive paper, there appeared no longer any doubt that an analogous process would succeed in copying the objects of external nature, although indeed they are much less illuminated.

Not having with me in the country a *camera obscura* of any considerable size, I constructed one out of a large box, the image being thrown upon one end of it by a good object glass fixed in the opposite end. This apparatus being armed with a sensitive paper, was taken out in a summer afternoon and placed about one hundred yards from a building favourably

illuminated by the sun. An hour or two afterwards I opened the box, and I found depicted upon the paper a very distinct representation of the building, with the exception of those parts of it which lay in the shade. A little experience in this branch of the art showed me that with smaller *cameræ obscuræ* the effect would be produced in a smaller time. Accordingly I had several small boxes made, in which I fixed lenses of shorter focus, and with these I obtained very perfect but extremely small pictures: such as without great stretch of imagination might be supposed to be the work of some Lilliputian artist. They require indeed examination with a lens to discover all their minutiæ.

In the summer of 1835 I made in this way a great number of representations of my house in the country, which is well suited to the purpose, from its ancient and remarkable architecture. And this building I believe to be the first that was ever yet known *to have drawn its own picture.*

The method of proceeding was this: having first adjusted the paper to the proper focus in each of these little *cameræ*, I then took a number of them with me out of doors and placed them in different situations around the building. After the lapse of half an hour I gathered them all up, and brought them within doors to open them. When opened, there was found in each a miniature picture of the objects before which it had been placed.

To the traveller in distant lands, who is ignorant, as too many unfortunately are, of the art of drawing, this little invention may prove of real service; and even to the artist himself, however skilful he may be. For although this natural process does not produce an effect much resembling the productions of his pencil, and therefore cannot be considered as capable of replacing them, yet it is to be recollected that he may often be so situated as to be able to devote only a single hour to the delineation of some very interesting locality. Now, since nothing prevents him from simultaneously disposing, in different positions, any number of these little *cameræ*, it is evident that their collective results, when examined afterwards, may furnish him with a large body of interesting memorials, and with numerous details which he had not had himself time either to note down or to delineate.

§ 10. *Delineations of Sculpture.*

Another use which I propose to make of my invention is for the copying of statues and bas-reliefs. I place these in strong sunshine, and put before them at a proper distance, and in the requisite position, a small camera obscura contain-

ing the prepared paper. In this way I have obtained images of various statues, &c. I have not pursued this branch of the subject to any extent; but I expect interesting results from it, and that it may be usefully employed under many circumstances.

§ 11. *Copying of Engravings.*

The invention may be employed with great facility for obtaining copies of drawings or engravings, or facsimiles of MSS. For this purpose the engraving is pressed upon the prepared paper, with its engraved side in contact with the latter. The pressure must be as uniform as possible, that the contact may be perfect; for the least interval sensibly injures the result, by producing a kind of cloudiness in lieu of the sharp strokes of the original.

When placed in the sun, the solar light gradually traverses the paper, except in those places where it is prevented from doing so by the opake lines of the engraving. It therefore of course makes an exact image or print of the design. This is one of the experiments which Davy and Wedgwood state that they tried, but failed, from want of sufficient sensibility in their paper.

The length of time requisite for effecting the copy depends on the thickness of the paper on which the engraving has been printed. At first I thought that it would not be possible to succeed with thick papers; but I found on trial that the success of the method was by no means so limited. It is enough for the purpose, if the paper allows any of the solar light to pass. When the paper is thick, I allow half an hour for the formation of a good copy. In this way I have copied very minute, complicated, and delicate engravings, crowded with figures of small size, which were rendered with great distinctness.

The effect of the copy, though of course unlike the original, (substituting as it does lights for shadows, and *vice versâ*,) yet is often very pleasing, and would, I think, suggest to artists useful ideas respecting light and shade.

It may be supposed that the engraving would be soiled or injured by being thus pressed against the prepared paper. There is not much danger of this, provided both are perfectly dry. It may be well to mention, however, that in case any stain should be perceived on the engraving, it may be readily removed by a chemical application which does no injury whatever to the paper.

In copying engravings, &c. by this method, the lights and shadows are reversed, consequently the effect is wholly altered. But if the picture so obtained is first *preserved* so as

to bear sunshine, it may be afterwards itself employed as an object to be copied; and by means of this second process the lights and shadows are brought back to their original disposition. In this way we have indeed to contend with the imperfections arising from two processes instead of one; but I believe this will be found merely a difficulty of manipulation. I propose to employ this for the purpose more particularly of multiplying at small expense copies of such rare or unique engravings as it would not be worth while to re-engrave, from the limited demand for them.

I will now add a few remarks concerning the very singular circumstance, which I have before briefly mentioned, viz. that the paper sometimes, although intended to be prepared of the most sensitive quality, turns out on trial to be wholly insensible to light, and incapable of change. The most singular part of this is the very small difference in the mode of preparation which causes so wide a discrepancy in the result. For instance, a sheet of paper is all prepared at the same time, and with the intention of giving it as much uniformity as possible: and yet, when exposed to sunshine, this paper will exhibit large white spots of very definite outline, where the preparing process has failed; the rest of the paper, where it has succeeded, turning black as rapidly as possible. Sometimes the spots are of a pale tint of cœrulean blue, and are surrounded by exceedingly definite outlines of perfect whiteness, contrasting very much with the blackness of the part immediately succeeding. With regard to the theory of this, I am only prepared to state as my opinion at present, that it is a case of what is called "unstable equilibrium." The process followed is such as to produce one of two definite chemical compounds; and when we happen to come near the limit which separates the two cases, it depends upon exceedingly small and often imperceptible circumstances, which of the two compounds shall be formed. That they are both definite compounds, is of course at present merely my conjecture; that they are signally different, is evident from their dissimilar properties.

I have thus endeavoured to give a brief outline of some of the peculiarities attending this new process, which I offer to the lovers of science and nature. That it is susceptible of great improvements, I have no manner of doubt; but even in its present state I believe it will be found capable of many useful and important applications besides those of which I have given a short account in the preceding pages.

An Account of the Processes employed in Photogenic Drawing, in a Letter to Samuel H. Christie, Esq., Sec. R.S., *from* H. Talbot, Esq., F.R.S.*

Dear Sir,—In compliance with the request of several scientific friends, who have been much interested with the account of the art of Photogenic Drawing, which I had the honour of presenting to the Royal Society on the 31st of last month, I will endeavour to explain, as briefly as I can, but at the same time without omitting any thing essential, the methods which I have hitherto employed for the production of these pictures.

If this explanation, on my part, should have the effect of drawing new inquirers into the field, and if any new discoveries of importance should be the result, as I anticipate, and especially if any means should be discovered by which the sensitiveness of the paper can be materially increased, I shall be the first to rejoice at the success; and in the meanwhile, I shall endeavour, as far as I may be able, to prosecute the inquiry myself.

The subject naturally divides itself into two heads; viz. the preparation of the paper, and the means of *fixing* the design.

(1.) *Preparation of the paper.*—In order to make what may be called ordinary photogenic paper, I select, in the first place, paper of a good firm quality and smooth surface. I do not know that any answers better than superfine writing paper. I dip it into a weak solution of common salt, and wipe it dry, by which the salt is uniformly distributed throughout its substance. I then spread a solution of nitrate of silver on one surface only, and dry it at the fire. The solution should not be saturated, but six or eight times diluted with water. When dry, the paper is fit for use.

I have found by experiment, that there is a certain proportion between the quantity of salt and that of the solution of silver, which answers best and gives the maximum effect. If the strength of the salt is augmented beyond this point, the effect diminishes, and, in certain cases, becomes exceedingly small.

This paper, if properly made, is very useful for all ordinary photogenic purposes. For example, nothing can be more perfect than the images it gives of leaves and flowers, especially with a summer sun: the light passing through the leaves delineates every ramification of their nerves.

Now, suppose we take a sheet of paper thus prepared, and wash it with a *saturated* solution of salt, and then dry it. We shall find (especially if the paper has been kept some weeks

* Read before the Royal Society, Feb. 21, 1839.

before the trial is made) that its sensibility is greatly diminished, and, in some cases, seems quite extinct. But if it is again washed with a liberal quantity of the solution of silver, it becomes again sensible to light, and even more so than it was at first. In this way, by alternately washing the paper with salt and silver, and drying it between times, I have succeeded in increasing its sensibility to the degree that is requisite for receiving the images of the camera obscura.

In conducting this operation it will be found that the results are sometimes more and sometimes less satisfactory in consequence of small and accidental variations in the proportions employed. It happens sometimes that the chloride of silver is disposed to darken of itself, without any exposure to light: this shows that the attempt to give it sensibility has been carried too far. The object is, to *approach* to this condition as near as possible without *reaching* it; so that the substance may be in a state ready to yield to the slightest extraneous force, such as the feeble impact of the violet rays when much attenuated. Having therefore prepared a number of sheets of paper with chemical proportions slightly different from one another, let a piece be cut from each, and, having been duly marked or numbered, let them be placed side by side in a very weak diffused light for about a quarter of an hour. Then, if any one of them, as frequently happens, exhibits a marked advantage over its competitors, I select the paper which bears the corresponding number to be placed in the camera obscura.

(2.) *Method of fixing the images.*—After having tried ammonia, and several other reagents, with very imperfect success, the first thing which gave me a successful result was the *iodide of potassium*, much diluted with water. If a photogenic picture is washed over with this liquid, an *iodide of silver* is formed which is absolutely unalterable by sunshines. This process requires precaution; for if the solution is too strong, it attacks the dark parts of the picture. It is requisite, therefore, to find by trial the proper proportions. The fixation of the pictures in this way, with proper management, is very beautiful and lasting. The specimen of *lace* which I exhibited to the Society, and which was made five years ago, was preserved in this manner.

But my usual method of fixing is different from this, and somewhat simpler, or at least requiring less nicety. It consists in immersing the picture in a *strong* solution of common salt, and then wiping off the superfluous moisture, and drying it. It is sufficiently singular that the same substance which is so useful in *giving* sensibility to the paper, should also be capable, under other circumstances, of *destroying* it; but such is, nevertheless, the fact.

Now, if the picture which has been thus washed and dried is placed in the sun, the white parts colour themselves of a pale lilac tint, after which they become insensible. Numerous experiments have shown to me that the depth of this lilac tint varies according to the quantity of salt used, relatively to the quantity of silver. But, by properly adjusting these, the images may, if desired, be retained of an absolute whiteness. I find I have omitted to mention that those preserved by *iodine* are always of a very pale primrose yellow; which has the extraordinary and very remarkable property of turning to a full gaudy yellow whenever it is exposed to the heat of a fire, and recovering its former colour again when it is cold.

I am, &c.

H. Fox Talbot.

XXXVIII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 141.]

Dec. 6, 1838.—A paper was in part read, entitled, "Experimental Researches in Electricity." *Fifteenth Series.*—"Note of the Character and Direction of the Electric Force of the Gymnotus." By Michael Faraday, Esq., D.C.L., F.R.S., &c.

Dec. 13, 1838.—The reading of a paper, entitled, "Experimental Researches in Electricity." *Fifteenth Series.**—"Note of the Character and Direction of the Electric Force of the Gymnotus." By Michael Faraday, Esq., D.C.L., F.R.S., &c., was resumed and concluded.

The author first briefly refers to what has been done by others in establishing the identity of the peculiar power in the Gymnotus and Torpedo with ordinary electricity, and then in reference to the intended conveyance to this country of Gymnoti from abroad, gives the instructions which he himself had received from Baron Humboldt for that purpose. A living Gymnotus, now in the possession of the Proprietors of the Gallery of Science in Adelaide Street, was placed for a time at the disposal of the author for the purpose of research, upon which he proceeded, with suitable apparatus, to compare its power with ordinary and voltaic electricity, and to obtain the direction of the force. Without removing it from the water he was able to obtain not only the results procured by others, but the other electrical phenomena required so as to leave no gap or deficiency in the evidence of identity. The shock, in very varied circumstances of position, was procured: the galvanometer affected; magnets were made; a wire was heated; polar chemical decomposition was effected, and the spark obtained. By comparative experiments made with the

* Prof. Faraday's preceding series of Exp. Res. in Electricity have been given, either entire or in abstract, in various volumes of Lond. and Edinb. Phil. Mag.—Edit.

animal and a powerful Leyden battery, it was concluded that the quantity of force in each shock of the former was very great. It was also ascertained by all the tests capable of bearing on the point, that the current of electricity was, in every case, from the anterior parts of the animal through the water or surrounding conductors to the posterior parts. The author then proceeds to express his hope that by means of these organs and the similar parts of the Torpedo, a relation as to *action* and *re-action* of the electric and nervous powers may be established experimentally; and he briefly describes the form of experiment which seems likely to yield positive results of this kind*.

Dec. 20, 1838.—A paper was read, entitled, "On the Curvature of Surfaces." By John R. Young, Esq. Communicated by John W. Lubbock, Esq., M.A., V.P. and Treas. R.S.

The principal object of this paper is, to remove the obscurity in which that part of the theory of the curvature of surfaces which relates to umbilical points has been left by Monge and Dupin, to whom, however, subsequently to the labours of Euler, we are chiefly indebted for a comprehensive and systematic theory of the curvature of surfaces. In it the author shows, that the lines of curvature at an umbilic are not, as at other points on a surface, two in number, or, as had been stated by Dupin, limited; but that they proceed in every possible direction from the umbilic.

The obscurity complained of is attributed to the inaccurate conceptions entertained by Monge and Dupin, of the import of the symbol $\frac{0}{0}$ in the analytical discussion of this question, the equation which determines the directions of the lines of curvature taking the form

$$0\left(\frac{dy}{dx}\right)^2 + 0\left(\frac{dy}{dx}\right) + 0 = 0$$

at an umbilic. After stating that Dupin has been guided by the determination of the differential calculus, the author remarks, that in no case is the differential calculus competent to decide whether $\frac{0}{0}$, the form which a general analytical result takes in certain particular hypotheses, as to the arbitrary quantities entering that result, has or has not innumerable values. He then states the principle, that those values of the arbitrary quantities (and none else) which render the equations of condition indeterminate must also render the final result, to which they lead, equally indeterminate; and that, therefore, when such result assumes the form $\frac{0}{0}$, its true character is to be tested by the equations that have led to it, after these have been modified by the hypothesis from which that form has arisen.

In a "Mémoire sur la Courbure des Surfaces," (Journal de l'École

* In the First Series of the Philosophical Magazine, vol. xv. p. 126, will be found a translation of E. Geoffroy's Memoir on the Anatomy of the Electrical Fishes, including that of the Gymnotus.—Edit.

Polytechnique, Tom. XIII.), Poisson has arrived at the conclusion, that the number of lines of curvature passing through an umbilical point is infinite, and that those selected by Dupin differ from the others only by satisfying an additional differential equation; those others equally satisfying the conditions of a line of curvature. These are precisely the conclusions arrived at by the author. As, however, he considers that the mode of investigation pursued by Poisson is peculiar and ill adapted to the objects apparently in view, namely, to reconcile the results of Monge and Dupin and to remove their obscurities, he was induced to investigate some of the more important properties of curve surfaces, by a method somewhat different from that usually employed.

Adopting $$Z = F(X, Y)$$
as the general equation of any surface; by attributing to X, Y, Z, increments x, y, z, and assuming that the axis Z coincides with the normal to the surface, or that the plane $x\,y$ is parallel to the tangent plane, an equation equivalent to, and nearly identical with, Dupin's equation of his indicatrix, is readily deduced. From this are immediately derived some properties of the radii of curvature, first shown by Dupin; and likewise the theorem of Meusnier. The author then enters upon the subject of the lines of curvature.

From the equations
$$A = 0, \qquad B = 0,$$
of the normal to the surface at a point on it, the equations of normal at a point near to the former are determined. That these normals may intersect, which is the condition giving the directions of the lines of curvature, the two sets of equations must simultaneously exist; and hence are deduced the differential equations of condition for the lines of curvature,

$$\frac{dA}{dx} + \frac{dA}{dy} \cdot \frac{dy}{dx} = 0, \qquad \frac{dB}{dx} + \frac{dB}{dy} \cdot \frac{dy}{dx} = 0.$$

By this method, which fundamentally is not very different from that of Monge, substituting the usual expressions for A and B, the equation that determines the directions of the lines of curvature is deduced, in the form in which it had been previously given by Monge and Dupin.

This final equation becoming at an umbilic of the form,

$$0\left(\frac{dy}{dx}\right)^2 + 0\left(\frac{dy}{dx}\right) + 0 = 0,$$

in which $\frac{dy}{dx}$ *may* be indeterminate, the author inquires how this indeterminate form will affect the equations of condition. As by this supposition, these are reduced to equations from which would result the conditions that would render all the coefficients of the determining equation 0, it is inferred that $\frac{dy}{dx}$ *must* be indeterminate, and that therefore, at an umbilic there issue lines of curvature in all directions.

Of these lines of curvature, it is possible that some may be distinguished from others, by proceeding from the point in more intimate contact with the osculating sphere, and it is therefore necessary to determine the analytical character of such particular lines of curvature. With this view, the author resumes the equation of the normal in the immediate vicinity of the umbilic. He then points out, that a straight line, whose equations contain the second differential coefficients, thus involving a new condition, will coincide more nearly with this normal, than can any straight line not having that condition. That the lines may intersect in the centre of the osculating sphere, their equations must simultaneously exist; and thus, that which most nearly coincides with the normal in the immediate vicinity of the umbilic has the new conditions,

$$\frac{d^2 A}{d x^2} + 2 \frac{d^2 A}{d x \, d y} \cdot \frac{d y}{d x} + \frac{d^2 A}{d y^2} \cdot \frac{d y^2}{d x^2} = 0,$$

$$\frac{d^2 B}{d x^2} + 2 \frac{d^2 B}{d x \, d y} \cdot \frac{d y}{d x} + \frac{d^2 B}{d y^2} \cdot \frac{d y^2}{d x^2} = 0,$$

in addition to the former ones.

From this it appears, that when the direction of a line of curvature issuing from an umbilic is such as to fulfil, besides the ordinary conditions, the foregoing new conditions, that line of curvature will lie more closely to the osculating sphere than any other not satisfying these additional equations. These new conditions arise from differentiating the preceding ones with respect to x and y, considered as dependent, regarding $\frac{d y}{d x}$ as constant; and as these are equivalent to a single condition (Monge's and Dupin's equation) it will be sufficient to differentiate this, under the above restrictions, in order to obtain a single condition equivalent to the new ones. As this single condition will appear under the form of an equation of the third degree in $\frac{d y}{d x}$, there will, in general, be at least one line of curvature, proceeding from the umbilic, of more than ordinary closeness to the osculating sphere; and there may be three. If, indeed, this equation of the third degree should, like that of the second from which it is deduced, be identical for the coordinates of the umbilic, it is obvious from the investigation, that we must then proceed to another differentiation; and so on, till we arrive at a determinate equation, the real roots of which will make known the number and directions of the lines of closest contact.

When, however, the author remarks in conclusion, all the lines of curvature issuing from the umbilic are equally close to the osculating sphere, then these successive differentiations will either at length exhaust the coefficients, and thus no determinate equation will arise; or else they will conduct to an equation whose roots are all imaginary: and one or other of these circumstances must always take place at the vertex of a surface of revolution.

The Society adjourned over the Christmas Recess to meet again on the 10th of January.

January 10, 1839.—A paper was read, entitled, "On the Laws of Mortality." By Charles Jellicoe, Esq. Communicated by P. M. Roget, M.D., Sec. R.S.

The author, considering that the variations and discrepancies in the annual decrements of life which are exhibited in the tables of mortality hitherto published would probably disappear, and that these decrements would follow a perfectly regular and uniform law, if the observations on which they are founded were sufficiently numerous, endeavours to arrive at an approximation to such a law, by proper interpolations in the series of the numbers of persons living at every tenth year of human life. The method he proposes, for the attainment of this object, is that of taking, by proper formulæ, the successive orders of differences, until the last order either disappears, or may be assumed equal to zero. With the aid of such differences, of which, by applying these formulæ, he gives the calculation, he constructs tables of the annual decrements founded principally on the results of the experience of the Equitable Assurance Society.

January 17.—A paper was read, entitled, "On the state of the Interior of the Earth." By W. Hopkins, Esq. A.M., F.R.S., Second Memoir. "On the Phenomena of Precession and Nutation, assuming the Fluidity of the Interior of the Earth*."

In this memoir the author investigates the amount of the luni-solar precession and nutation, assuming the earth to consist of a solid spheroidal shell filled with fluid. For the purpose of presenting the problem under its most simple form, he first supposes the solid shell to be bounded by a determinate inner spheroidal surface, of which the ellipticity is equal to that of the outer surface; the change from the solidity of the shell to the fluidity of the included mass being, not gradual, but abrupt. He also here supposes both the shell and the fluid to be homogeneous, and of equal density. The author then gives the statement of the problem which he proposes to investigate; the investigation itself, which occupies the remainder of the paper, being wholly analytical, and insusceptible of abridgement. The following, however, are the results to which he is conducted by this laborious process: namely, that, on the hypothesis above stated, 1. The Precession will be the same, whatever be the thickness of the shell, as if the whole earth were homogeneous and solid. 2. The Lunar Nutation will be the same as for the homogeneous spheroid to such a degree of approximation that the difference would be inappreciable to observation. 3. The Solar Nutation will be sensibly the same as for the homogeneous spheroid, unless the thickness of the shell be very nearly of a certain value, namely, something less than one quarter of the earth's radius; in which case this nutation might become much greater than for the solid spheroid. 4. In addition to the above motions of precession and nutation, the pole of the earth would have a small circular motion, depending entirely on the internal fluidity. The radius of the circle thus de-

* An abstract of Mr. Hopkins's First Memoir will be found in our Number for January, pres. vol. p. 52.—Edit.

scribed would be greatest when the thickness of the shell was the least: but the inequality thus produced would not, for the smallest thickness of the shell, exceed a quantity of the same order as the polar nutation, and for any but the most inconsiderable thickness of the shell would be entirely inappreciable to observation.

In his next communication, the author purposes considering the case in which both the solid shell and the inclosed fluid mass are of variable density.

"Apperçu sur une manière nouvelle d'envisager la théorie cristallographique dans le but d'établir les rapports de celle-ci avec la forme sphérique, ou elliptique, des molécules, ainsi qu'avec l'effet des milieux sur la forme cristalline." Par M. L. A. Necker. Communicated by P. M. Roget, M.D., Sec. R.S.

In this communication, after adverting to Haüy's theory of crystallization, in which the molecules are considered to be polyhedrons, to the views subsequently taken by Wollaston and Davy, and particularly to Brewster's conclusions, that there ought to be different forms of molecules, some spherical, some elliptical with two equal axes, and a third unequal to these, and others elliptical with three unequal axes; M. Necker states, that Mr. Dana is the only mineralogist who has attempted to introduce into crystallography the consideration of molecules with curved surfaces. Although, adopting the forms proposed by Brewster, and adding to them those of oblique solids, by introducing the idea of polarity in the axes of crystallization, Mr. Dana has successfully applied this molecular theory to crystallography, yet he goes no farther; and the most important and difficult steps in this branch of physical science still remain to be made, and many phenomena in crystallization, with the cause of which we are at present wholly unacquainted, still require to be explained by the theory. The author particularly refers to the important facts discovered by MM. Leblanc and Beudant, of the influence that solutions or mediums in which bodies crystallize have on the secondary forms which these bodies take; and states, that the present views of crystallography afford not even a glimpse of the least relation between such forms and the properties of the mediums. Why, he asks, does pure water appear, in general, to tend to simplify the forms, precisely as do certain mixtures, those of chlorite in axinite, quartz, felspar, &c., and why, on the contrary, do other mediums, acid or earthy, complicate them?

Impressed with the importance which must attach to the solution of such questions, M. Necker offers some ideas which long meditation on this important subject has suggested to him.

Adopting the ellipsoid as the form of the molecule, he remarks, that the more complicated the form of the crystal, the more the number of its faces increases, and the more, at the same time, does it approach to the ellipsoidal form of the molecule; and, on the contrary, the simpler the form becomes, the more does it recede from that with a curved surface. All crystalline forms may be considered as making a part of one or more series, which, in each system of crystallization, have for extreme terms, on the one side, the most sim-

ple solid of the system, or that which has the least number possible of faces, and on the other, the solid having the greatest number, namely a sphere or an ellipsoid. Although it is more convenient in the calculation of forms to start from the most simple polyhedral forms in order to arrive at the more complex, nothing proves that such has been the route which nature has followed. As long as we considered the integral molecules as polyhedral, it appeared natural to view them as grouping in polyhedrons; but when once we cease to admit polyhedral molecules, it then becomes most natural to suppose, that ellipsoidal molecules should have a tendency, more or less decided, to group in solids of the same form as themselves, when no extraneous circumstances interpose an obstacle to this tendency.

In order to give an idea of the kind of effect which would be produced on the form of the solid by these obstacles, such as the nature of the medium in which crystallization takes place, a hurried or tumultuous crystallization, &c., the author conceives that each molecule, as well as each solid formed by their union, has different axes of attraction, endued with different degrees of energy, and symmetrically disposed in groups, the weaker and the most numerous round the stronger, which are, at the same time, the smallest in number; all, in short, symmetrically arranged around the principal axes of crystallization, which are the most energetic of all. Thus we shall conceive that sort of polarity by which crystallization is distinguished from molecular attraction. The effect of obstacles, such as the attraction exerted by mediums, by interposed bodies, by the molecular attraction of the molecules themselves, when they arrive both in too great numbers and too rapidly towards the same point, will be the annihilation of the weaker axes; whence will follow the formation of a tangent plane to the spherical or elliptical surface. If the action of the obstacle goes on increasing, axes of attraction, which, by their intensity, had resisted the first obstacles, are destroyed by the new ones; and new tangential planes are produced, in which those that had been first formed finish by being confounded: thus it will happen that, by the increase of obstacles, the surface of the solid from being curved has become polyhedral, and finishes by presenting only an assemblage of a small number of plane faces, separated by edges, and placed tangentially at the extremity of the axes whose forces have longest resisted the action of the obstacles. But since the most energetic axes are necessarily the least numerous, the greater the energy they possess, the number of faces which bound the solid will continually decrease according as the obstacles increase; until, at length, the solid, reduced to its most simple form, no longer presents any but that constituted by the principal axes of crystallization, terminating at the summits of the solid angles of the simple polyhedron, which axes alone have been capable of withstanding the action of all the obstacles opposed to the tendency of the molecules to unite in the form of an ellipsoid.

On this hypothesis, the author explains how common salt, alum, sulphate of iron, &c., crystallize in pure water in the most simple forms, the reciprocal attraction of their molecules being controlled

and diminished by the affinity exerted on them by the molecules of the water; whilst if some of these molecules of water are neutralized by mixture with another soluble principle, they cease to act as an obstacle to the crystallization of the body, which then takes forms more complicated and approaching nearer to that of the normal solid with a curved surface.

M. Necker considers that the new views he has sketched require, for their complete developement, many ulterior details, as well as many new experiments and new facts; but that the tendency which the crystals of all systems present, to progress towards the curved surface form appropriate to each system, by the complication of their forces, is a fundamental fact of the first importance; and that an advance has been made by showing the bearing of the important experiments of MM. Leblanc and Beudant, and by having brought the theory of crystallography nearer to those views which the progress of chemistry and of physics have led us to adopt, relative to the form of the elementary molecules of bodies.

January 24.—A paper was read, entitled, "Experiments made on a piece of Peña silver, saved from the Lady Charlotte, wrecked on the coast of Ireland in December 1838, as to its capability of holding water." By W. D. Haggard, Esq. Communicated by Sir Henry Ellis, K.H., F.R.S.

Plata Peña, so called, is silver collected by quicksilver after the ore is pounded; it is then placed in a mould, and by great force the quicksilver is squeezed out, when it forms a mass, resembling dry mortar, of great porosity.

	Troy Weight. lbs.	oz.	dwts.	Decrease in weight. lbs.	oz.	dwts.
Original weight when taken from the box	38	10	0			
One day placed before the fire	37	0	15	1	9	5
Third day	35	5	0	1	7	0
Fifth day	34	5	5	0	11	15
Eighth day	34	0	2	0	5	3
Weight of water				4	9	3
				Increase in weight. lbs.	oz.	dwts.
Weight of the piece supposed to be quite dry	34	0	2			
First day from the fire	34	0	3	0	0	1
Third day	34	2	5	0	2	2
Fifth day	34	4	2	0	1	17
Eighth day	34	4	9	0	0	7
Gained in water from the air				0	4	7
Weight after water had been forced into it	39	1	19	4	9	10
Total weight of water contained in the piece				5	1	17

A paper was also read, entitled, "On the Application of the Conversion of Chlorates and Nitrates into Chlorides, and of Chlorides into Nitrates, to the determination of several equivalent numbers." By Frederick Penny, Esq. Communicated by H. Hennell, Esq. F.R.S.

The researches which form the subject of this paper were suggested by an inquiry into the most effectual method of ascertaining the quantity of nitrate of potassa existing in crude saltpetre. The author found that by the action of hydrochloric acid the nitrate of potassa was converted into the chloride of potassium; and conversely, that the chloride of potassium might, by the proper regulation of the temperature, be reconverted into the nitrate of potassa by the action of nitric acid. These mutual conversions afforded excellent means of determining, with great exactness, the relative equivalent numbers, in the theory of definite proportions, belonging to these salts, and to their respective constituent elements. The author, accordingly, pursued the investigation of these numbers by several successive steps, of which the details occupy the greater part of the present paper. He first determines the equivalent of chloride of potassium by decomposing chlorate of potassa into oxygen and chloride of potassium; the proportion between which gives the ratio which the respective equivalent numbers of each bear to one another, and also to that of chlorate of potassa. The equivalent of nitrate of potassa is next obtained by converting the chlorate and the chloride of potassium into that salt; and from these data the equivalents of chlorine and of nitrogen are deduced. A similar train of inquiry is next instituted with the corresponding salts having sodium for their base: chlorate of soda being decomposed into the chloride, and into the nitrate; nitrate of soda into chloride; and chloride of sodium into nitrate of soda. The results of these different series of experiments coincide so closely with one another as mutually to confirm their general accuracy in the most satisfactory manner. For the purpose of determining the equivalent numbers of the elementary bodies themselves, (namely, chlorine, nitrogen, potassium, and sodium,) the author employed the intermedium of silver, the several saline combinations of which with chlorine and with nitric acid were found to afford peculiar advantages for the accurate determination of the relative weights of the constituents of these salts, when subjected to various combinations and decompositions. The conclusions to which the author arrives with regard to the equivalent numbers for the six elementary bodies in question, tend to corroborate the views of the late Dr. Turner, and to overturn the favourite hypothesis that all equivalent numbers are simple multiples of that for hydrogen. He finds these numbers to be as follow:

Oxygen	8·
Chlorine	35·45
Nitrogen	14·02
Potassium	39·08
Sodium	23·05
Silver	107·97

The author intends to pursue these inquiries, by applying similar methods to the investigation of other classes of salts.

January 31.—A paper was read, entitled, "Some account of the Art of Photogenic Drawing, or the Process by which Natural Ob-

jects may be made to delineate themselves without the aid of the Artist's Pencil." By H. F. Talbot, Esq., F.R.S.

This paper is given, entire, in the present Number, p. 196.

February 7.—A paper was read, entitled, "Notice of a Shock of an Earthquake felt in the Island of St. Mary's, one of the Scilly Islands, on the 21st of January, 1839," in a letter addressed to the Secretary. By the Rev. George Wordley.

The tremulous motion of the ground is described as being very slight, and felt chiefly in the south parts of the island. It was accompanied by a peculiarly harsh and grating sound, which was only of momentary duration, and no particular agitation of the sea was observed.

A paper was in part read, entitled, "Observations on the Parallel Roads of Glen Roy, and of other parts of Lochabar, with an attempt to prove that they are of Marine Origin." By Charles Darwin, Esq., M.A., F.R.S., Sec. Geol. Soc.

GEOLOGICAL SOCIETY.

[Continued from p. 151.]

Dec. 9, 1838.—A paper on the "Phascolotherium," being the second part of the "Description of the Remains of Marsupial Mammalia from the Stonesfield Slate," by Richard Owen, Esq., F.G.S., was read.

Mr. Owen first gave a brief summary of the characters of the "Thylacotherium," described in the first part of the memoir*, and which he conceives fully prove the mammiferous nature of that fossil. He stated, that the remains of the split condyles in the specimen demonstrate their original convex form, which is diametrically opposite to that which characterizes the same part in all reptiles and all ovipara;—that the size, figure and position of the coronoid process are such as were never yet witnessed in any except a zoophagous mammal endowed with a temporal muscle sufficiently developed to demand so extensive an attachment for working a powerful carnivorous jaw;—that the teeth, composed of dense ivory with crowns covered with a thick coat of enamel, are every where distinct from the substance of the jaw, but have two fangs deeply imbedded in it;—that these teeth, which belong to the molar series, are of two kinds; the hinder being bristled with five cusps, four of which are placed in pairs transversely across the crown of the teeth, and the anterior or false molars, having a different form, and only two or three cusps—characters never yet found united in the teeth of any other than a zoophagous mammiferous quadruped;—that the general form of the jaw corresponds with the preceding more essential indications of its mammiferous nature. Fully impressed with the value of these characters, as determining the class to which the fossils belonged, Mr. Owen stated, that he had sought in the next place for secondary characters which might reveal the group of

* An abstract of the first part of Prof. Owen's memoir was given in our last Number, pres. vol. p. 141. Edit.

mammalia to which the remains could be assigned, and that he had found in the modification of the angle of the jaw, combined with the form, structure and proportions of the teeth, sufficient evidence to induce him to believe, that the *Thylacotherium* was a marsupial quadruped.

Mr. Owen then recapitulated the objections against the mammiferous nature of the *Thylacotherian* jaws from their supposed imperfect state; and repeated his former assertion, that they are in a condition to enable these characters to be fully ascertained: he next reviewed first the differences of opinion with respect to the actual structure of the jaw; and, secondly, to the interpretation of admitted appearances.

1. As respects the structure.—It has been asserted that the jaws must belong to cold-blooded vertebrata, because the articular surface is in the form of an entering angle; to which Mr. Owen replies, that the articular surface is supported on a convex condyle, which is met with in no other class of vertebrata except in the mammalia. Again, it is asserted, that the teeth are all of an uniform structure, as in certain reptiles; but, on reference to the fossils, Mr. Owen states, it will be found that such is not the case, and that the actual difference in the structure of the teeth strongly supports the mammiferous theory of the fossils.

2. With respect to the argument founded on an interpretation of structure, which really exists, the author showed, that the *Thylacotherium*, having eleven molars on each side of the lower jaw is no objection to its mammiferous nature, because among the placental carnivora, the *Canis Megalotis* has constantly one more grinder on each side of the lower jaw than the usual number; because the *Chrysochlore* among the *Insectivora* has also eight instead of seven molars in each ramus of the lower jaw; and the *Myrmecobius*, among the Marsupialia, has nine molars on each side of the lower jaw; and because some of the insectivorous *Armadillos* and zoophagous *Cetacea* offer still more numerous and reptile-like teeth, with all the true and essential characters of the mammiferous class. The objection to the false molars having two fangs, Mr. Owen showed was futile, as the greater number of the spurious molars in every genus of the placental *feræ* have two fangs, and the whole of them in the Marsupialia. If the ascending ramus in the Stonesfield jaws had been absent, and with it the evidence of their mammiferous nature afforded by the condyloid, coronoid and angular processes, Mr. Owen stated, that he conceived the teeth alone would have given sufficient proof, especially in their double fangs, that the fossils do belong to the highest class of animals.

In reply to the objections founded on the double fangs of the *Basilosaurus*, Mr. Owen said, that the characters of that fossil not having been fully given, it is doubtful to what class the animal belonged; and, in answer to the opinion, that certain sharks have double fangs, he explained, that the widely bifurcate basis supporting the tooth of the shark, is no part of the actual tooth, but true bone, and ossified parts of the jaw itself, to which the tooth is an-

chylosed at one part, and the ligaments of connexion attached at the other. The form, depth and position of the sockets of the teeth in the *Thylacothere* are precisely similar to those in the small opossums. The colour of the fossils, Mr. Owen said, could be no objection to those acquainted with the diversity in this respect, which obtains in the fossil remains of Mammalia. Lastly, with respect to the *Thylacothere*, the author stated, that the only trace of compound structure is a mere vascular groove running along its lower margin, and that a similar structure is present in the corresponding part of the lower jaw of some species of opossum, of the *Wombat*, of the *Balæna antarctica*, and of the *Myrmecobius*, though the groove does not reach so far forwards in this animal; and that a similar groove is present near the lower margin, but on the outer side of the jaw, in the *Sorex Indicus*.

Description of the Half Jaw of the Phascolotherium—This fossil is a right ramus of the lower jaw, having its internal or mesial surface exposed. It once formed the chief ornament of the private collection of Mr. Broderip, by whom it has since been liberally presented to the British Museum. It was described by Mr. Broderip in the Zoological Journal, and its distinction from the *Thylacotherium* clearly pointed out. The condyle of the jaw is entire, standing in bold relief, and presents the same form and degree of convexity as in the genera *Didelphys* and *Dasyurus*. In its being on a level with the molar teeth, it corresponds with the marsupial genera *Dasyurus* and *Thylacynus* as well as with the placental zoophaga. The general form and proportions of the coronoid process closely resemble those in zoophagous marsupials; but in the depth and form of the entering notch, between the process and the condyle, it corresponds most closely with the *Thylacynus*. Judging from the fractured surface of the inwardly reflected angle, that part had an extended oblique base, similar to the inflected angle of the *Thylacynus*. In the *Phascolotherium* the flattened inferior surface of the jaw, external to the fractured inflected angle, inclines outwards at an obtuse angle with the plane of the ascending ramus, and not at an acute angle, as in the *Thylacyne* and *Dasyurus;* but this difference is not one which approximates the fossil in question to any of the placental zoophaga; on the contrary, it is in the marsupial genus *Phascolomys*, where a precisely similar relation of the inferior flattened base to the elevated plate of the ascending ramus of the jaw is manifested. In the position of the dental foramen, the *Phascolothere*, like the *Thylacothere*, differs from all zoophagous marsupials, and the placental *feræ;* but in the *Hypsiprymnus* and *Phascolomys*, marsupial herbivora, the orifice of the dental canal is situated, as in the Stonesfield fossils, very near the vertical line dropped from the last molar teeth. The form of the symphysis, in the *Phascolothere*, cannot be truly determined; but Mr. Owen is of opinion that it resembles the symphysis of the *Didelphys* more than that of the *Dasyurus* or *Thylacynus*.

Mr. Owen agrees with Mr. Broderip in assigning four incisors to each ramus of the lower jaw of the *Phascolothere*, as in the *Didelphys;*

but in their scattered arrangement they resemble the incisors of the *Myrmecobius*. In the relative extent of the alveolar ridge occupied by the grinders, and in the proportions of the grinders to each other, especially the small size of the hindermost molar; the *Phascolothere* resembles the *Myrmecobius* more than it does the *Opossum*, *Dasyurus* or *Thylacynus;* but in the form of the crown, the molars of the fossil resemble the *Thylacynus* more closely than any other genus of marsupials. In the number of the grinders the *Phascolothere* resembles the *Opossum* and *Thylacine*, having four true and three false in each maxillary ramus; but the *molares veri* of the fossil differ from those of the *Opossum* and *Thylacothere* in wanting a pointed tubercle on the inner side of the middle large tubercle, and in the same transverse line with it, the place being occupied by a ridge which extends along the inner side of the base of the crown of the true molars, and projects a little beyond the anterior and posterior smaller cusps, giving the quinquecuspid appearance to the crown of the tooth. This ridge, which, in *Phascolotherium*, represents the inner cusps of the true molars in *Didelphys* and *Thylacotherium*, is wanting in *Thylacynus*, in which the true molars are more simple than in the *Phascolothere*, though hardly less distinguishable from the false molars. In the second true molar of the *Phascolothere*, the internal ridge is also obsolete at the base of the middle cusp, and this tooth presents a close resemblance to the corresponding tooth in the *Thylacine;* but in the *Thylacine* the two posterior molars increase in size, while in the *Phascolothere* they progressively diminish, as in the *Myrmecobius*. As the outer sides of the grinders in the jaw of the *Phascolothere* are imbedded in the matrix, we cannot be sure that there is not a smaller cuspidated ridge sloping down towards that side, as in the crowns of the teeth of the *Myrmecobius*. But, assuming that all the cusps of the teeth of the *Phascolothere* are exhibited in the fossil, still the crowns of these teeth resemble those of the *Thylacine* more than they do those of any placental *Insectivora* or *Phoca*, if even the form of the jaw permitted a comparison of it with that of any of the seal tribe. Connecting then the close resemblance which the molar teeth of the *Phascolotherium* bear to those of the *Thylacynus* with the similiarities of the ascending ramus of the jaw, Mr. Owen is of opinion that the Stonesfield fossil was nearly allied to *Thylacynus*, and that its position in the marsupial series is between *Thylacynus* and *Didelphys*. With respect to the supposed compound structure of the jaw of the *Phascolotherium*, Mr. Owen is of opinion that, of the two linear impressions which have been mistaken for *harmoniæ* or toothless sutures, one, a faint shallow linear impression continued from between the antepenultimate and penultimate molars obliquely downwards and backwards to the foramen of the dental artery, is due to the pressure of a small artery, and that the author possesses the jaw of a *Didelphys Virginiana* which exhibits a similar groove in the same place. Moreover, this groove in the *Phascolothere* does not occupy the same relative position as any of the contiguous margins of the opercular and dentary pieces of a reptile's jaw. The other impression in the jaw of

the *Phascolotherium* is a deep groove continued from the anterior extremity of the fractured base of the inflected angle obliquely downwards to the broken surface of the anterior part of the jaw. Whether this line be due to a vascular impression, or an accidental fracture, is doubtful; but as the lower jaw of the *Wombat* presents an impression in the precisely corresponding situation, and which is undoubtedly due to the presence of an artery, Mr. Owen conceives that this impression is also natural in the *Phascolothere*, but equally unconnected with a compound structure of the jaw; for there is not any suture in the compound jaw of a reptile which occupies a corresponding situation.

The most numerous, the most characteristic, and the best marked sutures in the compound jaws of a reptile, are those which define the limits of the coronoid, articular, angular, and surangular pieces, and which are chiefly conspicuous on the inner side of the posterior part of the jaw. Now the corresponding surface of the jaw of the *Phascolothere* is entire; yet the smallest trace of sutures, or of any indication that the coronoid or articular processes were distinct pieces, cannot be detected; these processes are clearly and indisputably continuous, and confluent with the rest of the ramus of the jaw. So that where sutures ought to be visible, if the jaw of the *Phascolothere* were composite, there are none; and the hypothetical sutures that are apparent do not agree in position with any of the real sutures of an oviparous compound jaw.

Lastly, with reference to the philosophy of pronouncing judgment on the saurian nature of the Stonesfield fossils from the appearance of sutures, Mr. Owen offered one remark, the justness of which, he said would be obvious alike to those who were, and to those who were not, conversant with comparative anatomy. The accumulative evidence of the true nature of the Stonesfield fossils, afforded by the shape of the condyle, coronoid process, angle of the jaw, different kinds of teeth, shape of their crowns, double fangs, implantation in sockets,—the appearance, he repeated, presented by these important particulars cannot be due to accident; while those which favour the evidence of the compound structure of the jaw may arise from accidental circumstances.

A paper was afterwards read, entitled "Observations on the Structure and Relations of the presumed Marsupial Remains from the Stonesfield Oolite," by William Ogilby, Esq., F.G.S.

These observations are intended by the author to embody only the most prominent characters of the fossils, and those essential points of structure in which they are necessarily related to the class of mammifers or of reptiles respectively. For the sake of putting the several points clearly and impartially, he arranged his observations under the two following heads:—

1. The relations of agreement which subsist between the fossils in question and the corresponding bones of recent marsupials and insectivora.

2. The characters in which the fossils differ from those families. Mr. Ogilby confined his remarks to marsupialia and insectivora,

because it is to those families only of mammifers that the fossils have been considered by anatomists to belong; and to the interior surface of the jaw, as the exterior is not exhibited in any of the fossil specimens.

1. In the general outline of the jaws, more especially in that of the *Didelphys* (*Phascolotherium*) *Bucklandii*, the author states, there is a very close resemblance to the jaw in recent insectivora and insectivorous marsupials; but he observes, that with respect to the uniform curvature along the inferior margin, Cuvier has adduced the same structure as distinctive of the Monitors, Iguanas, and other true saurian reptiles, so that whatever support these modifications of structure may give to the question respecting the marsupial nature of the Stonesfield fossils, as compared with other groups of mammals, they do not affect the previous question of their mammiferous nature, as compared with reptiles and fishes. The fossil jaws, Mr. Ogilby says, agree with those of mammals, and differ from those of all recent reptiles, in not being prolonged backward behind the articulating condyle; a character in conjunction with the former relation which would be, in this author's opinion, well nigh incontrovertible, if it were absolutely exclusive; but the extinct saurians, the *Pterodactyles, Ichthyosauri,* and *Plesiosauri,* cotemporaries of the Stonesfield fossils, differ from their recent congeners in this respect and agree with mammals. Mr. Ogilby is of opinion that the condyle is round both in *D. Prevostii* and *D. Bucklandii,* and is therefore a very strong point in favour of the mammiferous nature of the jaws. The angular process, he says, is distinct in one specimen of *D. Prevostii,* and, though broken off in the other, has left a well-defined impression; but that it agrees in position with the insectivora, and not the marsupialia, being situated in the plane passing through the coronoid process and the ramus of the jaw. In the *D. Bucklandii,* he conceives, the process is entirely wanting; but that there is a slight longitudinal ridge partially broken, which might be mistaken for it, though placed at a considerable distance up the jaw, or nearly on a level with the condyle, and not at the inferior angular rim of the jaw. He is therefore of opinion that the *D. Bucklandii* cannot be properly associated either with the marsupial or insectivorous mammals. The composition of the teeth, he conceives, cannot be advanced successfully against the mammiferous nature of the fossils, because animal matter preponderates over mineral in the teeth of the great majority of the Insectivorous *Cheiroptera,* as well as in those of the *Myrmecobius,* and other small marsupials. In the jaw of the *D. Prevostii,* Mr. Ogilby cannot perceive any appearance of a dentary canal, the fangs of the teeth, in his opinion, almost reaching the inferior margin of the jaw, and being implanted completely in the bone; but in the *D. Bucklandii,* he has observed, towards the anterior extremity of the jaw, a hollow space filled with foreign matter, and very like a dentary canal. The double fangs of the teeth of *D. Prevostii,* and probably of *D. Bucklandii,* he says, are strong points of agreement between the fossils and mammifers in general; but that double roots necessarily indi-

cate, not the mammiferous nature of the animal, but the compound form of the crowns of the teeth.

2. With respect to the most prominent characters by which the Stonesfield fossils are distinguished from recent mammals of the insectivorous and marsupial families, Mr. Ogilby mentioned, first, the position of the condyle, which is placed in the fossil jaws in a line rather below the level of the crowns of the teeth; and he stated that the condyle not being elevated above the line in the *Dasyurus Ursinus* and *Thylacinus Harrisii*, is not a valid argument, because those marsupials are carnivorous. The 2nd point urged by the author against the opinion, that the fossils belonged to insectivorous or marsupial mammifers, is in the nature and arrangement of the teeth. The number of the molars, he conceives, is a secondary consideration; but he is convinced that they cannot be separated in the fossil jaws into true and false, as in mammalia; the great length of the fangs, equal to at least three times the depth of the crowns, he conceives, is a strong objection to the fossils being placed in that class, as it is a character altogether peculiar and unexampled among mammals; the form of the teeth also, he stated, cannot be justly compared to that of any known species of marsupial or insectivorous mammifer, being, in the author's opinion, simply tricuspid, and without any appearance of interior lobes. As to the canines and incisors, Mr. Ogilby said, that the tooth in *D. Bucklandii*, which has been called a canine, is not larger than some of the presumed incisors, and that all of them are so widely separated as to occupy full five-twelfths of the entire dental line, whilst in the *Dasyurus viverrinus*, and other species of insectivorous marsupials, they occupy one-fifth part of the same space. Their being arranged longitudinally in the same line with the molars, he conceives, is another objection, because, among all mammals, the incisors occupy the front of the jaw, and stand at right angles to the line of the molars. With respect to the supposed compound structure of the jaw, Mr. Ogilby offered no formal opinion, but contented himself with simply stating the appearances; he, nevertheless, objected to the grooves being considered the impression of blood vessels, though he admitted that the form of the jaws is altogether different from that of any known reptile or fish.

From a due consideration of the whole of the evidence, Mr. Ogilby stated, in conclusion, that the fossils present so many important and distinctive characters in common with mammals on the one hand, and cold-blooded animals on the other, that he does not think naturalists are justified at present in pronouncing definitively to which class the fossils really belong.

ASTRONOMICAL SOCIETY.

Dec. 14, 1838.—The following communications were read:—

I. Extract of a letter from Professor Bessel to Sir J. Herschel, Bart., dated Konigsberg, Nov. 4, 1838*.

* Prof. Bessel's letter on the parallax of α *Cygni*, to which the above ex-

The means which I have employed to ascertain the effect of temperature upon the measures by the heliometer, consist in observing such of the stars of the *Pleiades* as are visible in the coldest winter, by night, and in the warmest summer, by day. Soon after the instrument was set up (in November and December 1829) I made a series of observations of this kind, and repeated them in the summer of 1830. From these I found the value of one revolution of the screw in the temperature f of Fahrenheit,

$$= 52''\cdot 91788 - (f - 49\cdot 2)0''\cdot 0004493$$ (*Astro. Nach.* No. 189, p. 418).

Further observations, however, have reduced the value of the last term of the formula to $0''\cdot 0003912$: this latter value is that which I have employed in the reduction of the observations of 61 *Cygni.* If this correction of the measures had been altogether neglected, the result, which the star a affords, would have been in error about $0''\cdot 06$; but, in the case of the star b, the effect would be altogether inappreciable, since the maximum of the influence in question takes place at that time of the year in which the parallax disappears. I owe this explanation to you, since you have inquired expressly as to this point; and, moreover, it could not be indifferent to me that an astronomer to whose opinion I attach so much importance, should not only be partially, but also thoroughly, satisfied as to the parallax of 61 *Cygni.*

After nine years' service, I resolved to take the heliometer to pieces, in order to examine anew all the parts of the mechanism of this very ponderous instrument; and to provide, in time, against any damage it may have sustained. The whole, however, is so solidly and durably constructed, that it has been found to need scarcely any repair. I have taken the opportunity of making some alterations for the greater convenience of the observer. The instrument, on account of these circumstances, has been nearly three weeks out of use; it is now, however, again in a fit state for observation.

I am particularly anxious to obtain your physical observations of the comet. Struve has lately communicated to me his own, which differ considerably from mine, as they show the tail defined, whereas it appeared to me undefined;  Struve,  Bessel. In other respects these observations are similar to mine, except that they go more into detail. It appears that I am the only one who has had the good fortune to be able to follow the comet during an entire night, in which the motion of the tail fell in its own direction. According to the letter which I had the pleasure to receive from you, the comet seems latterly to have lost its tail altogether; at least you mention only the complete definition of the disc, which also I consider a very important observation.

At the approaching disappearance of *Saturn's* ring, sufficiently

tract relates, will be found in our number for January, present volume, p. 68.—Edit.

powerful telescopes will probably be employed to show *all* the satellites of the planet. I believe that large reflecting telescopes will begin to supersede achromatic ones; at least, I have no doubt they are capable of greater perfection. They *can* be made with mathematical precision, which is not the case with achromatic telescopes. I think, also, that opticians would have devoted their attention to them in preference, if they had not been discouraged by their more rapid destructibility. If the method of making an indestructible *metallic* surface could be discovered, I should no longer doubt of a still further perfection of the reflecting telescope. Could not *hard* steel be made available? and would it not, if proper care was taken of it, be less destructible than the common metallic reflector?

II. Errors of Heliocentric Longitude and Ecliptic Polar Distance of the planet *Venus*, computed from the Tabular Errors of R. A. and N. P. D., given in the Cambridge Observations of 1836. By the Rev. R. Main.

Having been engaged in correcting the Elements of the Orbit of *Venus*, it occurred to Mr. Main that the reduction of the Tabular Errors of R. A. and N. P. D., derived from the Cambridge Observations of 1836, to errors of Heliocentric Longitudes and Ec. P. D., would form a desirable supplement to his papers.

He has added the equations which arise for the corrections of the Node and Inclination of the Orbit; and it is his intention to form those for the corrections of the four remaining elements.

He has used Mr. Airy's formulæ contained in the tenth volume of the Society's Memoirs, and divided the observations into groups of about the same length; applying the same corrections to the Right Ascensions for the Error of the Equinoctial Point.

The general agreement between the errors, and those given in the Greenwich Observations for 1836, shows the goodness of the observations, and gives additional confidence in the results to be derived from them.

The errors of the tables are then given, and equations for correcting the elements.

III. A Catalogue of 726 Stars, reduced to the year 1830, and deduced from the Observations made at Cambridge in the years 1828–1835. By G. B. Airy, Esq., Astronomer Royal.

The state of reduction in which the places of the stars had been published in each of the annual volumes of the Cambridge Observations, left little to be done for the formation of this catalogue, except the combination of the results of the different years. This was done by applying to the mean of each year's results the annual variation in the catalogue of this Society (except for stars near the pole), so as to bring the places up to Jan. 1, 1830; and then taking the mean of the different results for 1830, giving to each year a weight proportioned to the number of observations. Special methods of reducing some of the observations are fully explained in the preface, to which it is not requisite here to allude further; and a list of the principal discordances are subjoined, as well as of some of the observations that have been omitted in the reductions.

IV. Extract of a letter from Mr. Henderson to Mr. Baily, relative to the late Annular Eclipse of the Sun on May 15, 1836.

After correcting the places of the sun and moon, and their semidiameters, by the quantities mentioned in my last letter, I computed the beginning and end of the eclipse, and of the annulus; and I annex the *computed* times of these phænomena, and also the *observed.*

	Computed sid. Time. h m s	Observed Time. h m s
Beginning of eclipse	5 6 9·2	not observed.
At formation of annulus,		
First appearance of detached luminous portions of sun's limb		6 30 41·1
Internal contact of sun and moon	6 30 46·2	
Annulus completely formed by disappearance of black spots		6 30 50·1
At dissolution of annulus,		
Annulus broken by appearance of black spots		6 34 33·0
Internal contact of sun and moon	6 34 40·3	
Disappearance of detached luminous portions of sun's limb		6 34 44·0
End of eclipse	7 53 7·5	7 53 0·8 / 7·8

It thus appears that the true internal contacts happened between the moments when the beads of light were observed to appear and disappear, and when the black spots disappeared and appeared. At the formation of the annulus, when the beads appeared, $1''\cdot6$ of the moon (a segment of that *maximum* breadth) seems to have been beyond the sun's disc; and when the black spots disappeared, the annulus was $1''\cdot1$ in breadth, where least. This agrees with the appearance observed, "that the annulus was seen completely formed of sensible breadth at the narrowest part." Again, at the dissolution of the annulus, when the black spots appeared, it is found that the least breadth of the ring was $2''\cdot3$, also agreeing with the actual observation, that "the annulus, being of sensible breadth, was suddenly broken;" and when the beads disappeared, $1''\cdot2$ of the moon was beyond the sun's disc. The same telescope was used for observing the formation and dissolution of the annulus, and for making the observations from which the corrections of the elements have been determined.

On applying the same corrections, I find that in latitude 55° 27′ 30″ N. and longitude 10^m 12^s,0 W. of Greenwich, the true internal contacts were at

	Mean time. h. m. s.	Observed by you at Mean time. h. m. s.
	3 0 33	3 0 57
	3 5 7	3 5 23
Duration =	0 4 34	0 4 26

I suspect that the longitude of your station is not well determined; but, as a small error in the longitude will not sensibly affect the du-

ration of the annulus, I am inclined to believe that the times you noted are those when the black spots disappeared and appeared. You seem to have been in very nearly the line of the central eclipse, as the least distance of the centres was only 1″*.

V. A Letter from Mr. Lassell to the Rev. R. Sheepshanks, relative to Observations with a small Sextant.

The sextant, here alluded to, was made by Dollond. It is only 3 inches radius, divided to 20′, and by vernier reading to 30″; but, by means of the reading microscope, subdivisions may be estimated to 10″. The telescope magnifies 6 and 11 times; but the higher power is generally used. The whole packs in a box 4·3 inches square, and 2·7 inches deep. With this instrument Mr. Lassell made a number of observations on various stars, both for the time and latitude, for the express purpose of determining how near to the truth he might be able to approximate by its means. The observations are given in detail, and the result at which Mr. Lassell arrives is, that under ordinary circumstances the mean of one set of altitudes east, and another west, would give the time truly within about one second; and that a set of each, north and south, at something like equal altitudes, would give the latitude within eight or ten seconds.

FRIDAY-EVENING MEETINGS AT THE ROYAL INSTITUTION.

January 18th.—Mr. Faraday on the Gymnotus and Torpedo. The subject included a general view of the nature and condition of electric fishes, with the particular results which the speaker has lately communicated to the Royal Society.

January 25th.—Mr. Woodward on the apparatus for the public demonstration of the general laws and properties of polarized light.

February 1st.—Dr. Grant on the recent discoveries regarding the structure and history of animalcules.

February 8th.—Mr. Parsey on natural perspective.

February 15th.—Mr. Faraday on Gurney's oxy-oil, or Bude lamp for lighthouses and other situations.

XXXIX. *Intelligence and Miscellaneous Articles.*

ON THE CHLORO-CHROMIC ACID OF DR. THOMSON.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

IN the number of your Journal for July 1838, (Lond. and Edinb. Phil. Mag. vol. xiii. p. 78.) under the title of *Bichromate of Perchloride of Chromium,* there is an historical error which ought to be corrected. This beautiful substance was not discovered by Berzelius but in 1824 by Dr. Thomson of Glasgow, and described in the Phil. Trans. for 1827. It has since been examined by Unverdorben, Wöhler, Dumas, Rose, and Walter. The following are the formulæ which have been deduced from their analyses :—

* An abstract of Mr. Baily's observations on this eclipse appeared in Lond. and Edinb. Phil. Mag. vol. x. p. 230.—EDIT.

Thomson .... $Cr\ Cl_{2\frac{1}{2}}$ or $Cr\,O_{2\frac{1}{2}}\ Cl$
Wöhler...... $Cr\ Cl_3 + 2\,Cr\,O_{2\frac{1}{2}}$
Dumas ...... $Cr\ Cl_3$
Rose......... $Cr\ Cl + 2\,Cr\ O_{2\frac{1}{2}}$

There is an error also in the description of the properties of this compound. Chloro-chromic acid does not detonate with phosphorus unless that substance is moistened with water.

Your obedient servant,

June 29th, 1838. P.

FALL OF METEORITES IN SOUTH AFRICA.

"I have forwarded to Sir J. Herschel a splendid specimen of a meteor that exploded about 100 miles from Cape Town. The whole mass could not be less than 4 cubic feet. A pretty sort of solidification if it took place in our atmosphere; such an origin is scarcely conceivable. You will no doubt hear of it from him, together with its analysis*. We were at tea last Wednesday evening ($6^h\ 37^m$) broad daylight, when a meteor passed over the Observatory from the N.W., and appeared to fall a few hundred yards in front. We simultaneously jumped up. It was as brilliant as a full moon, but a more *pungent* light; no noise. The image of the window-sash upon the opposite wall was as bright as if the sun shone in front of it. The apparent size of the body was equal to that of a full moon."—*Extract from a letter* (*November* 25, 1838) *from Thomas Maclear, Cape Town, to Captain Smyth, R.N.*

ALLOXAN†.

The erythric acid of Brugnatelli; rediscovered by Wöhler and Liebig. One of the products of the decomposition of uric acid by nitric acid. One part of dry uric acid is added in successive portions to 4 parts of nitric acid of sp. gr. 1·45 to 1·5, by which it is dissolved with effervescence and the production of heat; the production of a high temperature must be avoided as much as possible by artificial cooling, and by adding the uric acid slowly. Small granular crystals of a strong lustre are thus formed, and by degrees the whole liquid is converted into a solid mass. This should then be placed in a glass funnel; and after the fluid parts have thus drained off, it should be spread upon a porous tile, where it is rendered perfectly dry. It is purified by solution in hot water and recrystallization.

Properties.—On the cooling of a warm but not perfectly saturated solution of alloxan, it is obtained in large colourless and transparent crystals of the right prismatic system, and of a strong adamantine lustre; these crystals effloresce rapidly, losing 25 per cent. =6 eq. water, and are converted when gently warmed, with the loss of

* From a letter received, together with a specimen of the meteorites, from Mr. George Thompson, of Cape Town, the date of the fall appears to have been October 13, 1838.—E.W.B.

† This and the seven following notices are copied from Part III. of the late Dr. Turner's Chemistry, recently published by Professor Liebig and Dr. Wilton Turner.

water, into anhydrous alloxan. If a hot saturated solution be allowed to crystallize in a warm place, anhydrous alloxan is deposited directly from the solution in oblique prisms, on the extremities of which truncated rhomboidal octohedrons are seen. It is very soluble in water, has a disagreeable odour, and a slightly saline astringent taste, reddens vegetable colours, and causes a purple stain on the skin. Treated with alkalies, alloxanic acid is formed; but on boiling it is decomposed into urea and mesoxalic acid. Heated with peroxide of lead it is decomposed into urea and carbonate of lead, with which a few traces of oxalate of lead are mixed. When brought into contact with zinc and hydrochloric acid, with chloride of zinc or sulphuretted hydrogen, alloxantin is produced; it is decomposed by free ammonia into mykomelinic acid, by nitric acid into parabanic acid, by sulphuric and hydrochloric acids into alloxantin, by sulphurous acid and ammonia into thionurate of ammonia, with alloxantin and ammonia into murexid. With a protosalt of iron and an alkali, it forms an indigo-blue solution. Does not unite without decomposition with the metallic oxides.

The formation of alloxan and the other products which arise at the same time, is dependent upon two perfectly independent decompositions; namely, upon the conversion of cyanoxalic acid into alloxan, and upon the mutual decomposition of urea and hyponitrous acid. To 1 eq. of cyanoxalic acid are added the elements of 4 eq. water, and 2 eq. oxygen from 1 eq. nitric acid, by which 1 eq. alloxan and 1 eq. hyponitrous acid are formed. The latter combines with the ammonia of the urea, and liberates cyanic acid; the hyponitrite of ammonia is decomposed by heat into nitrogen and water, and the cyanic acid with water is resolved into carbonic acid and ammonia, which unites with the free nitric acid.

$$
\begin{array}{ll}
\text{Cyanoxalic acid} = C_8N_2 & O_4 \\
\text{4 eq. water} & H_4O_4 \\
\text{2 eq. oxygen} & O_2 \\
\hline
\text{1 eq. alloxan} = C_8N_2H_4O_{10} & \\
\quad \text{Urea} = C_2N_2H_4O_2 & \\
\quad \text{Hyponitrous acid} = N \quad O_3 & \\
\hline
\end{array}
$$

$$
\begin{array}{ccccc}
C_2N_3H_4O_5 = & C_2O_4 + & N_2 + & NH_3 + & HO. \\
 & \text{Carbonic acid.} & \text{Nitrogen.} & \text{Ammonia.} & \text{Water.}
\end{array}
$$

It frequently happens that on dissolving the impure alloxan, for the purpose of purifying by a second crystallization, a portion of alloxantin is obtained; it may be easily separated from the alloxantin by cold water.

ALLOXANIC ACID.

Discovered by Wöhler and Liebig. Produced by the decomposition of alloxan by alkalies. It is prepared by decomposing alloxanate of baryta by sulphuric acid. A strongly acid fluid is obtained, which by gentle evaporation crystallizes in radiated groups of acicular crystals; it is a bibasic acid, dissolves zinc with the evolution of hydrogen, is unchanged by sulphuretted hydrogen, and precipitates the salts of silver, baryta, and lime. The anhydrous alloxanic

acid contains the constituents of half an equivalent of alloxan minus 1 eq. water.

Its formula is $C_8N_2H_2O_8 + 2$ eq.

ALLOXANIC ACID AND METALLIC OXIDES.

Alloxanic acid neutralizes the alkalies perfectly, decomposes the carbonates, and forms, when neutralized by ammonia, with the salts of silver a white precipitate, which by boiling becomes first yellow and then black, the change being accompanied by a rapid effervescence; treated with ammonia in excess, it produces white gelatinous precipitates with the salts of lime, strontia, and baryta; but the precipitate is redissolved by a large excess of water, and readily by an acid. The solutions of the neutral alloxanate of lime, strontia, and baryta, become turbid when boiled, the bases are precipitated, and urea and mesoxalic acid are formed.

Alloxanate of Baryta.—Prepared by adding barytic water to an aqueous solution of alloxan at the temperature of 140°; on each addition a white precipitate is formed, but it is redissolved by stirring; the barytic water is added in successive portions till the precipitate is permanent, when the solution is allowed to cool. The mother-liquor separated from the crystals is again to be heated and treated with barytic water as before, and this should be repeated as long as crystals are obtained.

Short transparent needles, or mother-of-pearl scales, which at 212° become milk-white and lose 3 eq. water; at 300° they are anhydrous; they are sparingly soluble in cold, but more freely in hot water; exposed to a red heat they leave a mixture of carbonate of baryta and cyanuret of barium.

Its formula is $C_8N_2H_2O_8, 2BaO + 8$ aq.

Alloxanate of Silver.—A white insoluble powder, which produces a slight explosion when heated; the residue after being heated to redness yields cyanic acid and metallic silver.

Its formula is $C_8N_2H_2O_8 + 2AgO$.

MESOXALIC ACID.

When a saturated solution of alloxanate of baryta or strontia is heated to the boiling point, a precipitate falls consisting of the carbonate, mesoxalate, and alloxanate of baryta or strontia. The solution, on evaporation, yields a crystalline crust, from which urea is separated by treating it with alcohol, and mesoxalate of baryta remains. If a solution of alloxan be added, drop by drop, to a boiling solution of acetate of lead, a very heavy granular precipitate of mesoxalate of lead is formed, and urea remains as the only other product in the solution. The mesoxalic acid may be obtained by decomposing this lead salt by sulphuric acid; it is a strongly acid solution, reddens vegetable colours, and forms, like the alloxanic acid, on the addition of ammonia, precipitates with the salts of baryta and lime, which are soluble in acids and a large excess of water; it may be boiled and evaporated without change. Its action on the salts of

silver is characteristic; it forms with them, after being neutralized by ammonia, a yellow precipitate, which on being gently heated is reduced to the metal with a rapid effervescence. The above-mentioned lead salt yields, on analysis, 80·4 per cent. of oxide of lead; it contains a slight admixture of a substance containing nitrogen, probably cyanate or cyanurate of lead, from which it cannot be perfectly purified. The composition of the lead salt is very probably expressed by the formula $C_3O_4 + 2PbO$, in which case its formation from alloxan and alloxanic acid admits of a ready explanation. From 1 eq. alloxan 1 eq. urea is separated, by which 2 eq. of anhydrous mesoxalic acid is left.

$$\begin{array}{rll} & \text{1 eq. alloxan} & = C_8N_2H_4O_{10} \\ - & \text{1 eq. urea} & = C_2N_2H_4O_2 \\ \hline = & \text{2 eq. mesoxalic acid} & = C_6 \qquad O_8 \end{array}$$

The above-mentioned mesoxalate of baryta contains 56 per cent. of baryta, from which its constitution is probably represented by the formula $C_3O_4 + \left\{\begin{matrix} BaO. \\ HO. \end{matrix}\right.$

MYKOMELINIC ACID.

Discovered by Wöhler and Liebig. Product of the decomposition of alloxan by ammonia. It is prepared by heating to 212 a solution of alloxan with an excess of ammonia, then neutralizing with an excess of dilute sulphuric acid and boiling for a few minutes. The mykomelinic acid falls as a yellow gelatinous precipitate, which dries to a yellow porous powder; it is with difficulty dissolved by cold, but more readily by hot water; the solution has a distinctly acid reaction; it decomposes the carbonated alkalies and is easily dissolved by the caustic alkalies, but on being boiled with them is decomposed with the evolution of ammonia; it forms, with the oxide of silver, a yellow compound, which is insoluble in water. It is produced by the decomposition of 1 eq. alloxan and 2 eq. ammonia into 1 eq. mykomelinic acid and 5 eq. water.

Its formula is probably $C_8N_4H_5O_5$.

PARABANIC ACID.

Discovered by Wöhler and Liebig. Product of the decomposition of uric acid and alloxan by nitric acid. Prepared by treating 1 part of uric acid, or 1 part of alloxan, in 8 parts of pretty strong nitric acid, evaporating to the consistence of a syrup, and allowing it to stand for some time, when it yields colourless crystals which may be purified by a second crystallization.

Properties.—Colourless, transparent, thin, hexagonal prisms; has a strong acid taste, very similar to that of oxalic acid; is very soluble in water, does not effloresce either in the air or in a warm room; fuses if heated, when a portion sublimes unchanged, but another part decomposes with the evolution of hydrocyanic acid. The cold solution neutralized by ammonia produces a white precipitate

in the salts of silver, which contains 70·62 per cent. of the oxide; when treated with ammonia it is converted into oxaluric acid.

It is formed by the decomposition of 1 eq. of uric acid, which, by the addition of 2 eq. of water and 4 eq. oxygen from the nitric acid, is resolved into 2 eq. carbonic acid, 1 eq. parbanic acid, and 1 eq. urea; the latter is decomposed as before-mentioned by the hyponitrous acid. One eq. alloxan with 2 eq. oxygen is solved into 2 eq. carbonic acid, 4 eq. water, and 1 eq. parabanic acid.

The formula of the crystalline acid is $C_6N_2O_4 + 2$ aq.

OXALURIC ACID.

Discovered by Wöhler and Liebig. Produced by the decomposition of parabanic acid. Prepared by adding dilute sulphuric or hydrochloric acid to a saturated solution of oxalurate of ammonia in hot water, and rapidly cooling the mixture when the oxaluric acid falls as a white crystalline powder; this should be washed with cold water as long as the washing, when neutralized by ammonia, causes with the salts of lime a precipitate which is perfectly redissolved by heat, or by an additional quantity of water. It is a white, or slightly yellow crystalline powder of an acid taste, reddens the vegetable colours, and, when neutralized by ammonia, forms with silver salts a white precipitate which is perfectly redissolved by boiling. By boiling in water it is completely decomposed into free oxalic acid and oxalate of urea. The oxaluric acid is formed by the addition of 2 eq. water to the constituents of the parabanic acid. It contains further the elements of 2 equivalents of oxalic acid and 1 eq. urea; it may be considered as uric acid in which the cyanoxalic acid has been replaced by the oxalic acid.

Its formula is $C_6N_2H_3O_7 +$ aq.

Oxalurate of Ammonia.—This salt may be formed by heating a solution of parabanic acid with ammonia, or more advantageously by treating a recently prepared solution of uric acid in dilute nitric acid with an excess of ammonia and evaporating. The liquid acquires at first a purple colour, which disappears during the evaporation, and if allowed to cool when arrived at a certain degree of concentration, it deposits radiated groups of hard acicular yellow crystals; they are obtained colourless by charcoal and recrystallization.

The oxalurate of ammonia crystallizes in radiated groups of fine acicular crystals, which have a silky lustre, and are readily dissolved by hot, but with difficulty by cold water; the solution has no reaction on vegetable colours, and may be boiled and evaporated without change; the dry salt loses no weight at 250°, but at a higher temperature it is decomposed with the rapid evolution of hydrocyanic acid. Acids separate from a concentrated solution the oxaluric acid as a crystalline powder.

Its formula is $NH_4O + C_6N_2H_3O_7$.

OXALURIC ACID AND METALLIC OXIDES.

The oxaluric acid forms with the alkalies very soluble, but with

the alkaline earth sparingly soluble salts. If concentrated solutions of oxalurate of ammonia, chloride of calcium or barium be mixed with each other, after standing some time, brilliant transparent scales or needles of oxalurate of baryta or lime will be deposited; a solution of the latter in water when treated with an excess of ammonia gives a basic salt in the form of a transparent gelatinous precipitate, which is redissolved by a large quantity of water.

Oxalurate of Silver.—This salt is obtained by mixing boiling solutions of oxalurate of ammonia and nitrate of silver, and is deposited as the solution cools in long anhydrous needles of a silky lustre; these are decomposed at a high temperature without explosion.

Its formula is $AgO + C_6N_2H_3O_7$.

PNEUMATIC TELEGRAPH.

A pneumatic telegraph has been invented by Mr. S. Crosley, an operative model of which is preparing for exhibition at the Polytechnic Institution. Atmospheric air is the conducting agent employed in its operation. The air is isolated by a tube extending from one station to another; each extremity of the tube being connected with a vessel containing a small volume of air in direct communication with the air in the tube. This vessel is employed as a reservoir to compensate for any increase or diminution which must necessarily arise from compression, or from changes in the temperature of the air, and for supplying any casual loss by leakage; the vessel must, therefore, be capable of enlargement and contraction in its capacity, after the manner of bellows, or as a gas-holder, by immersion in water, so as to maintain, uniformly, any particular degree of compression which may be given to it.

It will be evident to every one acquainted with the physical properties of atmospheric air, that if any certain degree of compression be produced and maintained in the reservoir, at one station, equilibrium will rapidly succeed, and the same degree of compression will extend to the opposite station, where it will become visible to an observer by means of a pressure Index.

Thus, with ten weights producing ten different degrees of compression, distinguished from each other numerically, and having a pressure index at the opposite station, marked by corresponding figures, any telegraphic numbers may be transmitted, referring in the usual way to a code of signals, which may be adapted to various purposes and to any language. The only manipulation is that of placing a weight of the required figure upon the collapsing vessel at either station, and the same figure will be represented by the index at the opposite station.

Previously to making a signal, the attention of the person, whose duty it is to observe it, is arrested by means of a preparatory signal.

The communications between one extremity and the other may be made known at intermediate stations, by connecting with the air tube, indexes corresponding with those at the extremities; but in

order to avoid the necessity for additional sounding apparatus, which would retard the communications between the extremities, it would be necessary to limit such intermediate communications to stated periods, so that an observer might be in attendance.

A trial was made with a tube of one inch in diameter, very nearly two miles in length, returning upon itself, so that both ends of the tube were brought to one place:—the compression applied at one end, was equal to a column of seven inches of water; and the effect on the index at the other end, appeared in fifteen seconds of time.

Laws have been propounded by eminent men on the expenditure of aeriform fluids through conduit pipes, and of the resistance of the pipes; but these are not strictly applicable to the present question. Under all circumstances, it seems desirable that experiments on a practical scale, at extended distances, should be resorted to, as the most satisfactory guide, for carrying into effect telegraphic communications of this kind.

METHOD OF DISTINGUISHING TRAP FROM BASALT.

Mons. H. Braconnot finds that these rocks may be distinguished by subjecting them to distillation; the trap always yields an empyreumatic ammoniacal product, which restores the colour of litmus paper reddened by an acid, whereas basalt produces no such effect; and he presumes that the organic matter which had existed in the materials of the basalt was destroyed by the volcanic heat by which the rock was produced; whereas he conceives trap to have been formed in water, under the influence of a moderate temperature, insufficient to destroy the organic matter which was contained in the debris from which it was formed. The trap was selected from various places, and of unquestionable nature; and the basalt was that of Clermont, in Auvergne. M. Braconnot has also found that various granites yield ammonia when heated; and the same effect was produced by serpentine and porphyry; but the gneiss of Freiberg, in Saxony, yielded an acid, which appeared to be the hydro-fluoric. Many other rocks of various kinds were subsequently found to yield ammonia.—*Annales de Chim. et de Phys., Jan.* 1838.

ABSORPTION OF AZOTE BY PLANTS DURING VEGETATION.

M. Boussingault has determined by numerous experiments, made with great care, that, while shooting, wheat and trefoil neither increase nor diminish the portion of azote which analysis shows them to contain; and that during germination, these grains lose carbon, hydrogen, and oxygen; and that each of these elements, as well as the proportions in which the loss occurs, varies at different stages of germination. It appears also, that during the cultivation of trefoil in soil absolutely deprived of manure, and under the influence of air and water only, this plant acquires carbon, hydrogen, oxygen, and a quantity of azote, appreciable by analysis: wheat cultivated exactly in the same circumstances, also takes from the air and water, carbon, hydrogen, and oxygen; but analysis does not prove that it has either lost or gained azote.—*Ibid.*

EXPLORATION OF THE INDIAN ARCHIPELAGO.

Much interest has of late been excited by the plan of exploring the Indian Archipelago made public in the Journal of the Geographical Society by Mr. James Brooke, who has since sailed on his adventurous and very meritorious undertaking. We deem it proper at this juncture to recall attention to the circumstance that a valued correspondent of one of the Editors of the Philosophical Magazine, in a paper published in the Annals of Philosophy, N.S., vol. xi. p. 178, urged on the Government and the public the importance of that scientific survey of the Indian Islands, which the zeal and enterprise of a private individual have now induced him to undertake. The paper is anonymous, and we do not feel at liberty therefore to state the name of the writer, unless we receive his permission to do so. But the views which are taken in it, are we conceive so just and accurate, and bear so directly upon the objects of Mr. Brooke's expedition, and also on those of some others now contemplated, that we think it highly worthy the perusal of our present readers.

Jan. 30, 1839.

ON THE INFLUENCE OF NATIVE MAGNESIA ON THE GERMINATION, VEGETATION, AND FRUCTIFICATION OF VEGETABLES. BY ANGELO ABBENE.

Among the various causes which produce barrenness in lands, has been enumerated the presence of magnesia, because it had been observed that the various magnesian soils are sterile. This opinion has begun to lose credit, since Bergmann, who examined the composition of fertile soils, considered magnesia as forming one of their principal constituents.

Prof. Giobert has performed a number of experiments to inquire into the action of native magnesia, which is found in numerous cultivated soils. In the environs of Castellamonte and of Baldissero, this substance is abundantly diffused in the soils cultivated with great success, and which exhibit a vigorous vegetation. There are many districts in Piedmont and elsewhere, where the bi-carbonate of lime and of magnesia is abundant in the cultivated lands, which produce beautiful plants. Giobert concluded from these experiments; 1st, that native carbonated magnesia is not injurious to the various functions of vegetables; 2nd, that on account of the solubility of magnesia in an excess of carbonic acid this earth can exercise an action analogous to that of lime; 3rd, that a magnesian soil may become fertile when the necessary manure is employed.

From these facts naturally proceeds the conclusion, that if the magnesia was dissolved in an excess of carbonic acid and water, and had entered like the lime into the composition of the sap, it ought to be found in the plants with the potash, lime, oxide of iron, &c. M. Abbene has ascertained this by the analysis of the ashes of plants which had grown in magnesiferous mixtures. Moreover, he endeavoured to find, by comparative experiments, whether the influence of magnesia on vegetation is analogous to that of lime. The following are the conclusions he arrives at: 1st, Native magnesia is not only not injurious to germination, vegetation, and fructification of plants, but on the contrary, appears to be favourable to these

functions. 2nd, Magnesia, being soluble in an excess of carbonic acid, has on vegetation an action analogous to that of lime; and when a soil contains magnesia not sufficiently carbonated, this defect may be remedied by the addition of manure, which by its decomposition furnishes the necessary quantity of carbonic acid; the amelioration will be much more efficacious if the soil be frequently disturbed, as then the air will better exercise its action. 3rd, When lime and magnesia exist in arable lands, the former is absorbed in preference by the plants on account of its greater affinity for carbonic acid. 4th, In barren magnesian lands, it is not to the magnesia that the sterility must be attributed, but to the cohesive state of their parts, to the want of manure, of clay, or of other composts, to the large quantity of oxide of iron, &c. 5th, Barren magnesian soils may be rendered fertile by means of calcariferous substances, as rubbish, chalk, ashes, marl, &c., provided the other conditions be fulfilled.—*Journal de Pharmacie* de Janvier, 1839.

METEOROLOGICAL OBSERVATIONS FOR JANUARY, 1839.

Chiswick.—Jan. 1. Overcast. 2, 3. Cloudy and fine. 4. Rain: clear. 5. Clear and very fine. 6. Overcast: sleet: rain at night, with wind increasing to a hurricane. 7. Boisterous. 8. Clear: slight snow. 9, 10. Frosty. 11. Overcast: rain. 12. Very fine. 13. Cloudy and windy, with slight showers. 14. Rain. 15. Very clear. 16. Fine, but cold. 17, 18. Sharp frost: clear. 19. Stormy and wet: clear at night, with aurora borealis. 20. Fine: rain. 21. Rain. 22. Clear and cold. 23. Overcast and fine. 24. Hazy: fine. 25. Fine. 26. Fine: slight snow. 27. Cloudy and cold. 28. Frosty: slight snow at night. 29. Clear: snow. 30. Sharp frost: slightly overcast: stormy with snow: tempestuous at night. 31. Snowing.

The hurricane, which commenced about 10 P.M., on the evening of the 6th, although considered unusually violent in the neighbourhood of London, appears to have done little damage, compared with the devastation occasioned a few hours previous in Ireland and in the west and north of England. In those parts of the kingdom forest trees that had withstood the storms of a century or centuries were torn up in thousands on some estates. There was here a slight frost in the morning; snow and sleet during the day, and rain in the evening. The wind was from south-east in the early part of the day, but towards night it blew from south-west and west.—R. Thompson.

Boston.—Jan. 1. Cloudy. 2. Stormy. 3. Fine: stormy night. 4. Cloudy: rain early A.M. 5. Fine. 6. Fine: snow P.M. 7. Stormy: blew a hurricane all day. 8. Stormy. 9, 10. Fine. 11. Cloudy; rain early A.M.: rain A.M. 12, 13. Cloudy. 14. Fine. 15. Stormy. 16, 17. Fine. 18. Fine: snow P.M. 19. Cloudy: rain early A.M.: rain P.M. 20. Cloudy: rain early A.M. 21. Rain. 22. Fine: snow early A.M. 23, 24. Cloudy. 25. Fine: rain P.M. 26. Cloudy: snow early A.M.: snow P.M. 27. Cloudy: rain A.M. 28. Cloudy. 29. Fine: rain and snow P.M. 30. Fine: rain P.M. 31. Cloudy: large fall of snow early A.M.: more snow in the day, with hail.

Applegarth Manse, Dumfries-shire.—Jan. 1. High wind and sharp showers. 2. Generally clear: occasional showers. 3. Very boisterous. 4. Calmed a little: stormy P.M. 5. Wind strong: snow. 6. Frost and snow: rain P.M. 7. Fearful storm: rain and sleet. 8. More calm: more snow. 9. Frost: snow lying three inches. 10. Thaw: snow melting. 11. Rain moderate: flood. 12. Moderate day: sunshine. 13. Frequent heavy showers. 14. Showery: aurora borealis. 15. Frosty after a boisterous night. 16. Clear frost: wind lulled. 17. Calm and frosty, and sunny. 18. Frost A.M.: rain at night. 19. Temperate: heavy flood. 20. Shower A.M.: still mild. 21. Frost A.M.: increasing P.M. 22. Clear frost: overcast P.M. 23. Frost again: slight thaw P.M. 24. Thaw: a few drops of rain. 25. Fine day, without frost. 26. Fine frosty day. 27. Calm and clear frost. 28. Frost: slight fall of snow. 29. Frost: storm of snow. 30. Frost: snow and high wind. 31. Frost: still snowing: nine inches deep.

Meteorological Observations made at the Apartments of the Royal Society by the Assistant Secretary, Mr. Roberton; *by* Mr. Thompson *at the Garden of the Horticultural Society at Chiswick, near London; by* Mr. Veall *at Boston, and by* Mr. Dunbar *at Applegarth Manse, Dumfries-shire.*

Days of Month. 1839. Jan.	Barometer.						Thermometer.									Wind.				Rain.				Dew-point.
	London: Roy. Soc. 9 a.m.	Chiswick.		Boston. 8½ a.m.	Dumfries-shire.		London: Roy. Soc.			Chiswick.		Boston. 8½ a.m.	Dumfries-shire.		London: Roy. Soc. 9 a.m.	Chiswick 1 p.m.	Bost.	Dumfries-shire.	London: Roy. Soc. 9 a.m.	Chiswick.	Boston.	Dumfries-shire.	Lond.: Roy. Soc. 9 a.m.
		Max.	Min.		9 a.m.	8½ p.m.	Fahr. 9 a.m.	Self-register. Max.	Self-register. Min.	Max.	Min.		9 a.m.	9 p.m.									
1.	30·342	30·391	30·081	29·80	30·00	29·87	40·8	41·4	36·2	50	43	43	44	42	s.	w.	w.	wsw.	...	...	...	...	35
2.	30·050	30·102	30·074	29·52	29·96	29·95	45·4	49·3	40·8	49	42	42	36	36½	w.	w.	nw.	wbys.	...	...	...	...	38
3.	29·944	29·958	29·672	29·41	29·70	29·25	45·3	46·2	44·2	49	40	41·5	44½	45	sw.	sw.	calm	w.	...	·04	...	...	40
4.	29·530	29·773	29·548	29·	29·24	29·43	42·0	47·2	41·9	47	31	43	37	35	s.	w.	calm	wbyn	·050	·01	·06	...	41
5.	29·682	29·795	29·641	29·42	29·41	29·30	38·4	44·5	35·6	44	32	34	34	32½	s.	sw.	w.	sw.	...	...	...	1·16	36
6.	29·718	29·735	29·147	29·30	29·55	28·99	35·7	39·0	34·2	53	32	31	20	34	s.	sse.	calm	sw.	·027	·33	...	...	31
☾ 7.	29·072	29·343	29·096	28·42	28·26	29·00	44·4	50·3	35·0	47	32	45	38	35	s.	w.	w.	wnw.	·227	·10	...	...	37
8.	29·538	29·592	29·526	29·	29·30	29·40	34·7	46·3	34·8	35	31	34	28	29½	w.	w.	n.	wnw.	·047	...	...	...	32
9.	29·716	30·210	29·735	29·36	29·78	30·10	34·2	39·8	33·6	36	21	27	20	22	nw.	nw.	w.	n.	...	...	...	...	31
10.	30·274	30·304	30·250	29·88	30·08	29·94	32·8	35·8	29·3	44	31	28	34	41	s.	s.	calm	s.	...	...	...	...	30
11.	30·150	30·181	30·180	29·60	29·80	29·90	42·4	43·2	32·6	50	42	44·5	43½	42	s.	sw.	sw.	sw.	·027	·06	·20	...	35
12.	30·180	30·232	30·202	29·70	30·09	30·03	47·7	50·0	42·0	51	39	43	40	38	nnw.	w.	calm	sw.	·044	...	·07	0·80	41
13.	30·020	30·049	29·994	29·45	29·69	29·80	49·8	50·3	42·7	52	41	50	46	41	sw var.	w.	w.	w.	...	·01	...	...	41
14.	29·968	29·970	29·781	29·42	29·75	29·63	44·9	51·8	43·2	46	34	41	39	34	sw.	w.	calm	sw.	...	·09	...	...	42
● 15.	29·818	30·033	29·850	29·27	29·68	29·95	36·6	47·7	36·0	44	31	36	36	33½	w.	nw.	w.	nnw.	·127	...	...	...	35
16.	29·944	30·024	29·951	29·55	30·00	30·06	35·3	43·2	33·0	41	26	30	32	29	w.	nw.	calm	nw.	...	...	...	...	32
17.	30·012	30·170	30·045	29·65	30·16	30·20	30·7	40·0	30·6	38	23	29·5	28	29	wnw.	nw.	nw.	nnw.	...	...	...	...	28
18.	30·172	30·204	30·074	29·80	30·19	29·80	29·7	38·0	29·5	44	27	27	21	37	w.	nw.	nw.	nnw.	...	...	...	...	29
19.	29·572	29·949	29·392	29·10	29·39	29·68	42·9	44·0	29·7	50	29	43·5	37	39	se.	sw.	sw.	n.	...	·13	·08	...	35
20.	30·008	30·038	29·729	29·55	29·82	29·71	38·6	47·8	35·0	51	39	39	37	39	sw.	sw.	calm	sw.	·177	·16	·09	...	33
21.	29·658	29·877	29·677	29·80	29·75	30·03	48·7	49·4	38·2	50	32	40	34	40	sw.	w.	calm	ne.	·175	·34	·50	...	38
☽ 22.	30·090	30·445	30·112	29·73	30·34	30·58	34·7	49·2	33·7	37	33	32	32½	32	n.	n.	calm	ne.	·333	...	·05	0·48	33
23.	30·530	30·551	30·494	30·15	30·60	30·55	36·2	37·4	34·6	42	30	34	29	36	nw.	nw.	n.	sw.	...	...	...	...	33
24.	30·532	30·573	30·308	30·12	30·53	30·40	36·2	40·4	33·7	48	37	36	40	40	sw.	nw.	calm	nw.	...	...	...	...	32
25.	30·088	30·108	29·989	29·65	30·13	30·20	43·0	43·8	36·2	49	32	43	39	38½	w var.	nw.	w.	nw.	...	...	...	...	37
26.	30·156	30·388	30·178	29·87	30·50	30·60	35·3	47·3	34·4	38	30	35	33	32	n.	ne.	calm	ne.	...	...	·05	...	32
27.	30·340	30·374	30·246	30·05	30·58	30·42	33·3	37·5	31·2	35	26	36·5	31	26	ne.	ne.	calm	n.	...	...	...	...	28
28.	30·006	30·064	29·682	29·73	30·03	29·74	32·7	35·6	31·6	36	29	32	27	30	nw.	ne.	calm	nw.	...	...	·07	...	29
○ 29.	29·548	29·591	29·155	29·22	29·50	29·29	33·2	37·3	31·6	39	17	29	27	22	w.	nw.	calm	sw.	...	...	...	...	28
30.	29·226	29·284	29·039	28·92	29·23	29·30	27·7	36·0	23·7	34	27	24	26	25	sw.	nw.	nw.	nw.	·194	...	·04	...	23
31.	29·150	29·729	29·141	28·83	29·58	29·97	30·5	32·3	25·5	37	22	29	31	30	w.	w.	nw.	n.	...	...	·19	...	23
Mean.	29·904	30·033	29·804	29·49	29·790	30·450	38·2	43·3	34·7	43·74	31·64	36·1	33·6	34·7					Sum. 1·428	1·27	1·40	2·44	Mean. 33·5

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XL. *Classification of the Older Stratified Rocks of Devonshire and Cornwall.* *By the Rev. Professor* Sedgwick, *F.R.S. F.G.S., and* Roderick Impey Murchison, *Esq., F.R.S. F.G.S.**

IT was the general belief of geologists, when we commenced our examination of Devonshire, in the summer of 1836, that the larger portion of its area was occupied by greywacke or transition rocks of high antiquity, from which the culm-bearing strata of Bideford, and many other places in the county, could not be separated. Having occupied ourselves for some years in deciphering the relative age of the older rocks of England and Wales, we were naturally anxious to apply to this county those principles of classification by which the successive subdivisions of the Silurian, and (though much more imperfectly) also of the Cambrian System, had been determined; for it seemed to us very anomalous, that the culm beds of Devonshire, though stated to resemble those of the coal field of Pembrokeshire, both in their mineral characters and in their associated fossil plants, should be interpolated among the most ancient greywacke rocks of the county. Mr. De la Beche had, however, in a communication to the Geological Society, stated that such was their position; and he completed an Ordnance geological map, in which all the culm rocks, as well as all the so called greywacke rocks of Devon, were represented under one uniform colour. This map we purchased many months before we commenced our examination of Devonshire. An outline of the results of this examination was laid before the British Association in the autumn of the same year; and we exhibited a section from the north coast to Dartmoor, (copied, though not quite correctly, in the

* Communicated by the Authors.

Athenæum,) in which we pointed out an ascending series of ancient stratified rocks, the greater part then supposed to belong to the upper Cambrian, and a comparatively thin band to the lower Silurian strata: and we further showed that the culm measures of Devon, so far from being mere courses subordinate to these older rocks, constituted, in fact, a vast basin or trough of carbonaceous deposits, differing from the rocks on which they rested, in mineral character and in organic remains. We also exhibited a map in which the boundaries of this great carbonaceous trough were defined, with as much accuracy as the short period of our examination permitted; and we stated our belief, that at least all the upper part of it was the equivalent of the true coal measures. We further proved, that the granite of Dartmoor had been protruded after the consolidation of the culm or coal formation. Subsequently, we embodied these results with other details, not presented to the British Association, in a second memoir, read before the Geological Society (June, 1837), in which we gave a short account of the structure of South Devon; the different rocks of which we placed in parallel with their equivalents in North Devon, referring them, as well as we were able, to their types in other districts of Great Britain.

Adhering strictly to our first view, viz. that the great overlying culm-bearing trough is the equivalent of the carboniferous system; we proceed to point out the reasons which induce us to make a material change in the classification and equivalents of the older rocks of Devon and Cornwall.

North Devon.—In our previous communications we subdivided the part of Devon which lies between the north coast and the trough of culm deposits into five ascending mineral masses, closely linked to, or passing into one another. From its lithological character, from the ferruginous impressions of stems of Encrinites, and the resemblance of certain casts of shells (much distorted, however, by compression, and by lines of slaty cleavage) to fossils of the Caradoc sandstone, we placed the fifth group in the lower part of the Silurian system; consequently the four inferior groups (though different in many respects from anything we had ever seen among the older rocks of Wales or Cumberland) necessarily fell into the upper portion of the Cambrian system. Over all these groups came the culm measures, and certainly without the intervention of conglomerates or any manifest discordancy of position.

Since our memoirs were read, Mr. Weaver examined the neighbourhood of Barnstaple; and confirming our views as to the age of the culm-measures, reported them to be unconformable to the older rocks*. One of the authors, for the ex-

* Proceedings of the Geol. Soc. vol. ii. —.

press purpose, among others, of inquiring into this point, revisited the country last summer, but was not able to discover any discordance in the junction south of Barnstaple: on the contrary, the culmiferous deposits seemed rather to be a continuous uninterrupted series, following the older rocks. Now, if this be so, one of two things must follow. Either the culm measures must be older, or the rocks of North Devon younger, than we had at first supposed. The question then is this, what is the evidence given by the fossils? The fossil plants of the culm beds are undistinguishable from those of Pembrokeshire, the nearest coal field, while certain shells of the black limestone, subordinate to the lower culm strata, prove also to be undistinguishable from species which occur in the true carboniferous system; and thus we have on fossil evidence every ground for believing that our first view respecting the culm measures was correct, and that they are the true equivalents of the carboniferous system.

On re-examining, however, the collections of fossils we had made among the strata which lie beneath the northern edge of the culm field, we have seen reason to change our views respecting the age of these older rocks.

In the uppermost of the five groups into which we provisionally divided those rocks, we now find no unequivocal lower Silurian fossils: for though two distorted casts have much the appearance of shells of that date, there are other and better preserved specimens, which approach so near to species known in the carboniferous limestone, (*Spirifer cuspidatus* and *Spirifer attenuatus*) as to be almost undistinguishable from them; and with these are found *Leptænæ*, having somewhat the character of upper Silurian fossils, and undescribed *Terebratulæ*, together with *Trilobites*, some of entirely new forms, and others approaching to upper Silurian types.

Below these slaty and calcareous strata near Barnstaple, a part of which we first mistook, as above stated, for Caradoc sandstone*, are the sandstones of Baggy Point, Marwood, and Sloley, which we have elsewhere described in detail. In the line of these are found certain fossil plants, specimens of which were first sent to us by Major Harding, and others were laid before the Geological Society by the Rev. D. Williams: some of them are considered by Mr. Williams and Mr. De la Beche, on the authority of Dr. Lindley, to be undistinguishable from

* The strong resemblance of the *Caradoc sandstone*, in consequence of mineral character and the circular marks of crinoidal stems, to the sandstones of the lower shale of the *Carboniferous limestone*, and the probability that this resemblance would lead to mistakes, has already been pointed out by one of the authors. (Silurian System, pp. 384, 453.)

plants of the carboniferous system. On the same line are ferruginous bands, with casts of shells, communicated by Major Harding, which strongly reminded us of forms in the old red sandstone; one indeed being, as Mr. Sowerby thinks, identical with the *Bellerophon globatus* of that system *.

In the next underlying groups of Morte Bay and Ilfracombe, we have few well-preserved mollusca, but among them is a very wide *Spirifer* and a spined *Productus*, approaching to the carboniferous type, but unlike anything we know in the Silurian or Cambrian rocks. Corals occur in parts of these Ilfracombe groups, and among them is the *Favosites polymorpha*, a species most abundant in the upper Silurian rocks, but not found in the lower.

Lastly, in the lowest group we perceived certain large heart-shaped forms, quite unlike anything we had ever seen until we afterwards detected the same in the collection of Mr. Hennah, from the Plymouth limestone: and in the very lowest fossil beds near Linton, we still perceived some of the same specific forms that occur near Barnstaple; which, though undescribed, may be pointed out as of characters intermediate between those of the Silurian and carboniferous fossils, the balance of evidence inclining rather to the younger of the two types; there being few if any traces of the genus *Orthis* so eminently characteristic of the Silurian system. These evidences all tend one way, and (confirmed as they are by the passage of the bottom culm-measures) force us to believe that the oldest rocks of North Devon are much younger than we at first supposed: and coupling these with other proofs still more cogent from South Devon, we arrived at the conclusion which we shall presently explain.

South Devon.—In the communication of 1837 to the Geological Society, we described several sections from the granite of Dartmoor to the south coast of Devon, and (omitting the altered slates) we divided the older stratified rocks into three groups; the lowest, composed of slates, containing subordinate bands of the Ashburton limestone, and ending in ascending order with the Plymouth and Torbay limestones, which we considered to be identical; the second, composed of red sandstone, with occasional subordinate beds of shale, &c.; the third of soft non-fossiliferous schists, extending almost to Start Point. These we considered (and we believe correctly) of the same age with the groups of North Devon; and we attempted (without the aid of fossils) to bring them into close comparison, by identifying the great deposits of *red sandstone* of the two districts. This identification of the separate groups we considered as merely provisional, "to be con-

* Murchison's Silurian System, Pl. 3, 15.

firmed or rejected," to use our own words, "by the examination of the organic remains of the several groups."

And here we may briefly allude to visits made by one of the authors to different parts of this region, both before and after the Bristol Meeting of the British Association, and subsequent to the reading of our second memoir in London. During the autumn of 1836, he traced the calcareous system of South Devon into Cornwall, and followed it continuously through the north of the Lizard into Mount's Bay, and at Looe, Fowey, and other places, found numerous organic remains (not however yet described): in a former year, 1828, he had traced the fossil slates of Tintagel into Padstow Bay, but had then no time to carry his observations further south. Before the autumn of 1836, Mr. De la Beche had worked out the structure of the north side of Cornwall in great detail: and one of us, informed by him of the existence of many other fossil localities, examined the north Cornish coast; and concluded, in a paper read at Cambridge in the winter of 1836–37, *that the fossiliferous system on both sides of Cornwall was the same*, and therefore of the age, or nearly so, of the calcareous rocks of South Devon. The same view was re-stated by us in the paper read to the Geological Society. We still believe this view to be correct, and hesitate not to class the calcareous rocks of South Devon, and the fossiliferous slates of both coasts of Cornwall, together.

Last summer (1838), one of us paid a visit to Devon and the neighbourhood of Launceston, for the purpose of ascertaining the following points:

1. Whether the limestones of Newton Bushell could be classed with the carboniferous or mountain limestone, an opinion advanced by our friend Mr. Austen *, who drew that conclusion from the forms of the numerous fossils he had brought to light. 2. Whether the true culm-measures pass under the Ashburton or Chudleigh limestone. 3. To ascertain (especially after Mr. Weaver's memoir) whether there was any general discordancy of position between the culm-bearing beds and the Cornish slates near Launceston. To each of these questions his observations gave a negative. He was confirmed in the views first stated by the authors in 1837, concerning the relations of the limestone of Torbay to that of Plymouth; and notwithstanding the many fossils of the Newton Bushell limestone resembling those of the carboniferous system, its beds formed clearly a part of the group subordinate to the great southern slate deposit. He saw no good reason for thinking that the culm-measures pass under any

* See Proceedings of Geological Society.

part of the limestones and slates of South Devon: and lastly, he found near South Petherwin what appeared an *unequivocal passage* between the fossiliferous slates and the overlying culm series. From these facts it follows; 1st, that the fossiliferous slates of Barnstaple, on the north side of the great culm trough, must be nearly of the same age as those of South Petherwin on its southern side; for certainly the inferior strata of the culm series are the same, or very nearly the same, at the two localities; 2nd, either that the culm series was older, or the fossiliferous slate of North Cornwall newer than we had supposed in our memoir of June 1837. Under these circumstances, it became doubly important to examine large suites of fossils, before we could arrive at a correct conclusion as to the true place of the older Devonian strata in the geological series*.

The most extensive collection of fossils which had been made in South Devon, were from the Plymouth limestone by the Rev. R. Hennah, and from the limestones of the neighbourhood of Newton Bushell by Mr. Austen. The latter was sent to the Geological Society to illustrate a memoir by that gentleman; and the inspection of its contents convinced Mr. Lonsdale, that few, if any, of the organic remains could be strictly identified with species of the carboniferous limestone to which Mr. Austen compared them; for although there were some which had a close resemblance, still there were many which bore the impress of a distinct type, while others, particularly the corals, seemed to approach to certain forms of the upper Silurian group; and hence Mr. Lonsdale was

* The fossils of South Petherwin, from the first, presented a great difficulty. One or two of them very nearly resembled mountain lime fossils; and as a group they were *not* identical with any series we had before examined. This induced one of the authors to join Mr. Austen (in July, 1837) in an excursion to that neighbourhood; thinking it possible that the South Petherwin limestone *might* form a part of the base of the culm-measures. An unseasonable interruption compelled them, after two or three days, to leave the country: but they ascertained, 1st, that the limestone in question did not form a part of the culm series; and 2nd, that the fossils of South Petherwin, &c., were, as a group, nearly the same with the fossils of South Devon; thus confirming a previous conclusion (drawn from less perfect evidence), that the fossiliferous systems of South Devon and of Cornwall were the same. They also found one junction (since visited and sketched by Mr. De la Beche, Report, &c. p. 107.) in which the culm beds appeared to rest unconformably on the older slates. This kind of junction seems to form the exception and not the rule; and does not, we think, invalidate the statement made above. Many such junctions might indeed be found in the very heart of the culm-measures, where the beds are all of one age. Such an appearance of want of conformity does not therefore invalidate the fact of a true *passage* from the Petherwin slates into the culm-measures.

induced to suggest to us (now more than a year ago), that the South Devon rocks would be found to occupy an intermediate place between the carboniferous and the Silurian systems. The collection of Mr. Hennah was unfortunately missent to Cambridge, and was only unpacked very recently: but upon its examination (within these few weeks) by one of the authors, in company with Professor Phillips and Mr. James Sowerby, the same general results were arrived at; namely, the existence of some fossils undistinguishable from certain forms of the carboniferous limestone, and others from those of the upper Silurian rocks, while many were entirely new. Again, the corals of this limestone being examined by Mr. Lonsdale, gave nearly the same results as those of Mr. Austen.

After again examining our own collections, and looking to the fossils of North Devon, South Devon and Cornwall as a whole, we distinctly perceived that Mr. Lonsdale was right, and that they must all belong to a system intermediate between the two great systems (Silurian and Carboniferous) which had so recently been shown to be entirely separated from each other, both by their order of superposition and their imbedded organic remains.

The publication, indeed, of the Silurian system and its numerous fossils affords us a fixed term in the series of the older rocks; and the previous labours of Professor Phillips and others having made us acquainted with the organic remains of the carboniferous system, we have now, for the first time, the means of placing the Devonian groups in their true order.

Without entering, on this occasion, into specific details, we may state that the zoological groups of the Devonian rocks are all of characters intermediate between those which mark the Carboniferous and Silurian epochs. Thus, for example, among the Cephalopoda, *Goniatites* have hitherto been considered as typical of the carboniferous system, while the researches of one of the authors have shown that they never occur in the Silurian system. They do, however, appear in some of the older Devonian rocks; and, just as we should expect, they are associated with analogues of an entirely new type, the *Endosiphonites**.

Again, there are many large and broad *Spirifers* in these Devonian rocks, which closely approach to the forms of that genus, so abundant in the carboniferous system. But this genus is feebly developed in the Silurian system, and the few species that do occur are entirely unlike the large typical Spirifers of the carboniferous æra; while the *Orthis*, or real Silurian

* See Trans. of the Cambridge Phil. Soc., vol. vi.

Spirifer, is rarely if ever seen in Devonshire. The large round spinose *Producti* are among the best-marked fossils of the carboniferous system. Now, the closest researches have not hitherto brought to light the existence of one species having this character in the Silurian system; while in Devonshire we find several associated with other species, which are analogous both to the Silurian and carboniferous types. On the other hand, the families and genera which predominate so much more in the Silurian than in any other system, viz. *Trilobites* and *Orthoceratites*, are here just of the intermediate character which ought to be detected in deposits connecting that system with the carboniferous. Some of them approach very closely to upper Silurian species, if indeed there be not some undistinguishable; while others, particularly some of the Trilobites, are of forms entirely different from any species hitherto found, either in the Silurian or Carboniferous systems*.

In regard to the corals, Mr. Lonsdale informs us, that the few which he can identify with published species (the most abundant and certain being *Favosites polymorpha*, *Porites pyrimorphis*, and *Stromatopora concentrica*), belong to the upper Silurian rocks; while there are several which are new and undescribed. Again, the chain coral (*Catenipora escharoides*), and many of the most remarkable Silurian types, are entirely absent, nor has a single species common to the carboniferous limestone been yet detected among the numerous polypifers of South Devon.

Whether, in the sequel, we shall present to the public a suite of engravings of all the undescribed Devonian and Cornish fossils which we have collected, or which have been lent to us, or shall consign them to Professor Phillips, to complete a task for which he is so eminently qualified, and for which purpose he is, we are glad to learn, to be employed by the Government, is of little moment: but after such evidences, we have no hesitation in putting forth our present classification, and in accepting, in the broadest form, the conclusions to which the general view of these organic remains lead us, viz. *that the oldest slaty and arenaceous rocks of Devon and Cornwall are the equivalents of the old red sandstone.* We also place, in the same parallel, the older rocks of North Devon; being now fortified in our conclusions by the evidence of the fossils, by the sections, and by the order of superposition; which indicates, on both sides of the great carbonaceous trough, a passage downwards from the carboniferous system (the horizon of which

* Our friend M. de Verneuil acquaints us, that having examined a collection of South Devon fossils sent to him by Mr. Austen, he is of opinion that seven or eight of these shells are undistinguishable from fossils of the Eifel, which he refers to the Silurian system.

we consider as fixed,) into the Devonian equivalents of the old red sandstone.

Under this view, the supposed difficulty arising out of the existence of certain species of fossil plants, both in the carboniferous system and "undisputed greywacke rocks*," is at once obviated: for the hard brown and greenish-grey micaceous sandstones between Ilfracombe and Barnstaple, in which these plants were discovered†, are now placed in the upper part of the old red sandstone, in which all true analogy would impel us to look for the presence of some of the species of plants common to the carboniferous epoch: and we are strengthened in this conviction by the evidence of the shells on the same line with these plants, among which is a species, as before stated, of *Bellerophon*, identical, as far as casts can prove it, with a shell figured from the old red sandstone. In regard to the organic remains of the old red sandstone, one of the authors has indeed already published those forms which mark its passage into the Silurian system. Aware of the enormous thickness of this arenaceous series in the British Isles, and having ascertained, to a certain extent, the peculiarity of its fossils, particularly of its fishes, he proposed that it should be considered "*a system*," intermediate between the Carboniferous and Silurian systems‡.

Whilst, however, he indicated the existence of certain shells connecting the old red and Silurian systems, as well as fossil fishes of very peculiar types in the former, he knew too well that the greater mass of this vast system, particularly all its upper members, contained no organic remains in the countries which he illustrated. In stating that "the strata (Carboniferous and Silurian), so broadly distinguished by their organic remains, are separated by accumulations of enormous thickness, and that the vast time occupied in their deposit accounts satisfactorily for an almost entire change in the forms of animal life;" he also thus declared his anticipation concerning the old red system: "We are yet unprovided with zoological links to connect the *whole series*, though I have no doubt that such proofs will be hereafter discovered, and that we shall then see in them as perfect evidences of a transition between the old red and carboniferous rocks, as we now trace from the Cambrian, through the Silurian into the old red system§."

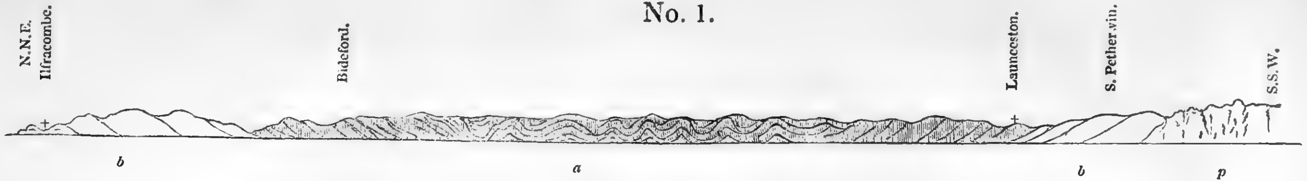
* See De la Beche's Report, p. 132, *et seq.*

† The plants were first observed by Major Harding and the Rev. D. Williams.

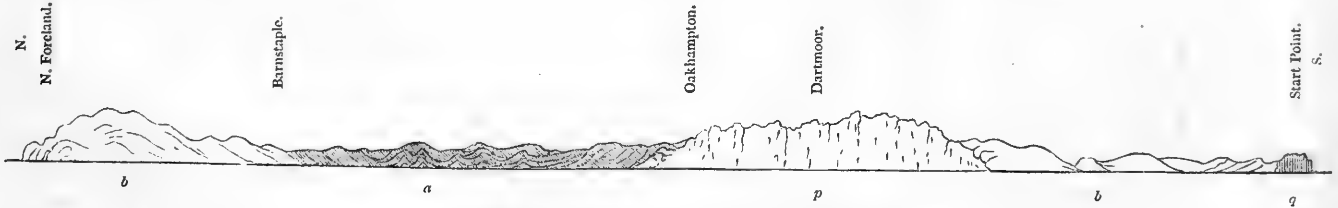
‡ See Silurian System and Map.

§ Murchison's Silurian System, p. 585.

No. 1.



No. 2.



a. Culmiferous Rocks .... = Carboniferous System.
b. Slaty Rocks, Sandstone and Limestone = Old Red System.

p. Granite.
q. Metamorphic Rocks?

This hiatus is at last, in a great measure, filled up by the fossils of the older rocks of Devon and Cornwall; and believing our views to be correct, we thus represent Devonshire in two general sections from north to south.

In the higher of these the carboniferous trough is seen to repose at each side on the slates and calcareous sandstones of the old red system.

In the lower section the culm trough is flanked on the north side only by the slaty rocks of the old red system, the granite of Dartmoor having been protruded on its southern edge; while the old red system reappears again in the southern part of the county, terminated by a band of micaceo-chloritic schists, which are perfectly parallel to the great disturbing axis of Cornwall and Devon, and are probably altered or metamorphic strata.

In propounding these views, we have no desire to conceal the error into which we were first led by trusting too much to mineral characters. We unfortunately never gave, till very lately, that attention to the organic remains which is indispensable; but having been the first to point out, that nearly one half of Devonshire is the equivalent of the carboniferous rocks, we have no hesitation in going a step further, guided by a closer inspection of the organic remains, and by the apparent fact, that this carboniferous tract passes downwards into the older system. It is not the first time that we (and we believe we may say every practical geologist who has examined old rocks) have been deceived in attributing too high antiquity to strata having an antique lithological aspect and a slaty cleavage: but the day is now passed, when such features, still less the mere colour or composition of rocks, can be allowed to lead to any true estimate of their age. We have red sandstones and conglomerates among the slates of the Cambrian system; red and variegated sandstones abundant in the lower Silurian rocks, as well as in the greatest red systems of our islands, the one underlying, the other overlying, the carboniferous deposits.

If it be contended that the old red sandstone of Great-Britain, as hitherto understood, presents a more or less uniform character in its range from the Highlands of Scotland into South Wales, we must qualify the assertion. The system assumes very great mineral varieties of aspect in different regions: in some tracts (parts of Scotland and Cumberland) it is usually composed of thick, coarse conglomerates, while in others such masses give way to the finest laminated sandstone and shale. In the north-western districts of the Highlands, so completely was an eminent geologist* misled

* Dr. MacCulloch.

by its lithological structure, that he classed a large portion of it as *primary sandstone*,—an opinion which was afterwards corrected by an examination of that region by ourselves*. Again, it has fallen to us to show, that the black bituminous schists of Caithness and the Orkneys form an integral part of the old red sandstone: and what trace of lithological resemblance, we would ask, is there between such rocks as these, which occupy the north-eastern extremity of our island, and the great masses of the same deposit which prevail in the form of the variegated marls and concretionary limestones of Herefordshire and Brecknockshire? Passing from the latter district into Pembrokeshire, we perceive the old red sandstone rapidly changing its aspect and composition. The concretionary limestones disappear; and a hard, brownish-red, schistose rock replaces the soft marls, and large tracts are occupied by yellow and grey, hard, siliceous sandstones.

As, therefore, such great lithological changes actually occur in continuous strata within such a limited area, why may we not believe (particularly when we have strong collateral reasons for doing so depending both on fossils and sections,) that the older fossiliferous rocks of Devonshire and Cornwall are the equivalents of the old red sandstone? They have indeed, to a great extent, assumed a new mineral type—we say to a great extent; for we ourselves, in both our previous memoirs, have described many of the rocks in question as *much resembling* the old red sandstone.

Various causes may have co-operated in producing the peculiar mineral character of the Devonian and Cornish strata. Among these, igneous action is the most apparent, and the country it is well known bristles with rocks of igneous origin, many of which are well described by Mr. De la Beche: and if (as we believe) he is correct in supposing that many of these rocks were formed contemporaneously with the strata among which they appear, we are furnished with one of the conditions under which the mineral character of a part of this region of the old red system has been greatly modified. But, independently of any supposed igneous action, what is there among the analogies of sedimentary deposits which should not lead us to embrace the view, that formations of the same epoch may have completely distinct mineral types? Has it not been over and over again demonstrated that the limestones of any particular series are often represented by siliceous sandstones, even at the opposite extremities of our island, though the order in which the organic remains occur is precisely the same in these different rocks? The sandy and shaly strata of the coast of

* See Geol. Trans., vol. ii. p. 125.

Yorkshire and the hard siliceous fossil grits of Brora, proved to be of the same age as the Oxfordshire oolites, are among the many striking illustrations of a phænomenon, of the prevalence of which, indeed, no stronger proof can be given, than that geologically and zoologically considered, the massive clays of the London basin are the same as the white limestones and hard siliceous grits of Paris.

In applying, therefore, these analogies to Devonshire we should say, that if the true mountain limestone (under its ordinary aspect) thins out and is no longer traceable, we ought to look for its equivalent in sandstone and shale: and in accounting for the great development of many marine animals which appear in the Devonian limestones, we have, *à priori*, reason to expect the appearance of large stratified masses of calcareous matter.

If these views be confirmed by our best fossil conchologists (of which we have little doubt), then will they have a most important bearing upon the classification of the ancient rocks of foreign countries, and we believe also of Ireland. In large tracts of Europe, the first great series under consideration, (to which one of us applied the word "system,") is supposed to be wanting: but if this supposed absence be founded chiefly on mineral characters, the representative of the system may still be discovered by its typical organic remains, though enveloped in rocks like these of Devon and Cornwall, or in strata still further removed from what we have been in the habit of regarding as the general type*.

Conclusion.—We had no intention a few weeks ago of writing upon this subject. It is true that we had been gradually changing some of our views respecting the age of the oldest rocks of Devon and Cornwall (since the suggestion of our friend Mr. Lonsdale before alluded to); and we should soon have placed our opinion upon record before the Geological Society. The publication, however, of the Report upon the Geology of Devon and Cornwall, seen by one of the authors for the first time within these few days, compels us, in justice to our character †, (for it is now not merely a scientific, but also

* We are led to believe, from the data already before us in the works of foreign authors, that the old red system will be found in the provinces of Russia and the Scandinavian countries, as well as in Poland and Germany. M. Elie de Beaumont has recently written to one of the authors, and approving of the establishment of the old red sandstone as a separate "System", he says that he has no doubt it will be largely found on the continent.

† At the conclusion of a note affixed to p. 130 of Mr. De la Beche's Report on the Geology of Cornwall and Devon occur the following words: "He," the Rev. D. Williams, "observes, that he stated the fact of the car-

a moral question) to state immediately and concisely what our views have been and what they now are; in order that our scientific brethren (with whom our statement cannot fail to produce its proper effect,) may have it in their power to draw a just conclusion as to the part we have taken in the new classification of the rocks of Devonshire and Cornwall. If any one should think that there is somewhat of a polemic spirit either in this page or those which follow, we request him to bear in mind, that the determination of the great *culm trough of Devon* and the settlement of its true geological position, is *the key to the whole structure of the two counties*; and that no one was in possession of this key, until in 1836, we offered it to the British Association at Bristol.

We pass over the circumstance alluded to in the note below, trusting that Mr. De la Beche is incapable of insinuating that which he knows to be incorrect; and we shall conclude this sketch with a short statement of facts in relation to his operations and our own. We have already stated, that before we entered upon an examination of Devonshire, Mr. De la Beche had exhibited a map to the Geological Society which he said was complete, and which was afterwards on sale for some months. It contained many excellent details, the result of the labours of former years, and on the whole was justly considered to be of great value: but it made no separation of the culm-bearing or carbonaceous strata from the older rocks. Before our visit to Devon, this author had, in fact, neither

bonaceous rocks occurring *in a trough*, bounded by the ridge of Exmoor on the north, and the granite of Dartmoor and slates of Foerabury and Boscastle on the south, at the Meeting of the British Association held at Dublin, in 1835. (Report of the Proceedings of the British Association, Oct. 7, 1837.)" We are compelled to give an unequivocal contradiction to this statement. We were both present at Dublin when the paper alluded to was read, and we took part in the short discussion by which it was followed; and we assert that the author, the Rev. D. Williams, considered the fossil plants he exhibited from Devonshire, as derived from the oldest greywacke rocks of the district, and yet identical with those of the true coal-field of Pembroke. He described no section, and made no allusion whatever to the existence of any overlying carbonaceous trough. This assertion is perhaps superfluous on our part, for we have only to appeal to all the geologists who were present at the Dublin Meeting, and to refer others who were not there, to the abstract prepared by Professor Phillips from Mr. Williams's *short notice*, for such it really was. An assertion of Mr. Williams, made in the autumn of 1837, and which till this time we never saw, is put forth to establish his pretension to a discovery said to have been announced to the British Association in 1835; while neither the official records of the Association, nor, as far as we can learn, any journal of that year, make the slightest allusion to such a circumstance! The facts speak for themselves. We are the first persons who pointed out the existence of the carbonaceous trough; and we never received the smallest hint of the sort from Mr. D. Williams.

separated the culm measures as a distinct formation, nor had he ascertained their place in the general section; for he had so far mistaken their relations, on their northern limits, as to place them *not over* (as he now does after our example), but *under* the first three groups of the *older* system; and their southern limits he had never ascertained. In short, to work out this point it was necessary he should do what he had *not* done—to separate the black or culm limestones from the other limestones of Devon; for without this it was impossible for any one to take the first step. As soon as our new views were announced, he suppressed the map as originally coloured, and revisited Devonshire to make himself acquainted with the fresh data, which he had now no difficulty in doing. Major Harding had, indeed, pointed out to one of us the existence of the culm limestone even as long ago as 1835, at the time when Mr. De la Beche's map was first exhibited. Great therefcre was our surprise when we perceived that no distinctions were drawn in the ordnance geological map between this very remarkable flat-bedded limestone, and those limestones of a slaty character which predominate in other parts of Devon; the more so as the fossils of the one are entirely distinct from those of the other. So palpable, however, is the line of demarcation between the rocks containing the black or culm limestone and those containing the slaty limestone, that Major Harding, though then very slightly acquainted with geological phænomena, had traced the boundaries of the two classes of deposit from Barnstaple by Swimbridge and Venn before we entered Devonshire, calling the one *mountain* and the other *transition limestone*, and had thus prepared an excellent point of departure for our examination. On the south side of the trough (as we afterwards showed it to be), Mr. De la Beche referred the inferior part of the culm series to the oldest system of Devon and Cornwall; because it alternated, like the Cornish killas, with certain contemporaneous trap rocks. We pointed out to him the imperfection of this reasoning, because similar alternations take place among rocks of many ages, and therefore by themselves prove nothing. Now, in 1839, Mr. De la Beche publishes his Report, accompanied by an index map, in both of which he adopts our view respecting the right mapping of the culm measures as a distinct formation, which he calls "carbonaceous rocks:" and although before our visit he had always insisted on these rocks forming an integral part of the greywacke series, he now gives valid reasons for their separation, yet without acknowledgement, and with no other indication that we were the agents who produced this change in his

views, except the announcement, in a few words, of what occurred at Bristol, from the perusal of which no one could deduce any correct inference as to what we really had done. A single sentence, a mere parenthesis (*if to the point*) would have satisfied us; and would not merely have been right, but would have been prudent.

Whatever may be, even the small merit of our labours in Devonshire, this at least we affirm, *that they are perfectly original.* In passing from the north coast to the south, across the whole county, we were astonished at the novelty and unexpected nature of the phænomena that successively rose before us, contrasted with every thing we had before understood from those who had examined Devonshire. We were amazed when, after having ascertained the separation of the culm-measures from the underlying slaty rocks, we stood upon the cliffs of Clovelly, and found ourselves compelled by the evidence before us to sketch them in as the highly inclined masses of a *coal-field*, dipping away from the more ancient strata of North Devon: and still greater was our surprise when, following those cliffs by Bude we found the same carbonaceous or culmiferous system still rolling over in countless striking flexures, till passing into North Cornwall it rises up against and rests upon the older slates of that county. Nor again were we less surprised on finding the same system lifted up, penetrated, and altered by the granite of Dartmoor. So far from any attempt having been previously made to effect this great separation, the position even of the several limestone bands in North Devon had been mistaken by Mr. De la Beche, and considered by him as the repetition of the *same calcareous group* by successive undulations. The truth is, that no one can make a correct section among slaty rocks till he learns to distinguish cleavage from stratification; and it is astonishing how very few geologists are even at this time masters of the subject. This is we believe the explanation of some of Mr. De la Beche's early difficulties, and may be the reason why he did not first separate the culmiferous deposits (as he now does) from the *greywacke* of Devonshire.

Should any one ask what we effected in Devon, we reply,—

1. That we were the first to give anything resembling a correct section of the mineral masses between the N. and S. coasts of Devon; and till such a step was taken, it was impossible to commence any classification of the subordinate groups.

2. That we determined the relative place and succession of the distinct calcareous and fossiliferous groups.

3. That we proved the existence of the culm limestones on

both sides of a great trough, and included the hitherto anomalous limestones of Holcombe Rogus in the same carboniferous system.

4. That we ascertained the subdivisions of the culm series, and laid down, for the first time, on a map, its extent (with one limited and perhaps doubtful exception at its south-eastern extremity near Ashburton), thus converting the supposed *ancient greywacke* rocks of nearly one-half of the large county of Devon into equivalents of the carboniferous system, as it is already represented in two published geological maps*.

In attempting to classify the oldest rocks of Devon we fell into some false conclusions from imperfect data. These we have corrected, and the whole series of Devonshire and Cornwall is now, we trust, exhibited in harmony; the lower sandstones and slates being the equivalents of the old red sandstone, the next natural group beneath the great coal-bearing strata of the British Isles, and the whole being exhibited under peculiar mineral types.

In asserting that the older stratified rocks of Devonshire and Cornwall are upon a broad scale the equivalents of the *carboniferous and old red systems*, we do not however deny that in some tracts the lowest members of these rocks may represent the *upper division of the Silurian System*: for although we have as yet found few if any of the fossils most typical of that system, we admit that when the sediments of a given epoch have been accumulated under peculiar conditions, we must expect to find considerable variations in the forms of animal life. Again, we know that the older rocks of this region have undergone great changes in assuming their present hard and slaty character; and under such circumstances the difficulty of precisely limiting the boundary line of any given portion of them is prodigiously increased. In tracing, for example, the Silurian System from its typical region into the sea-cliffs of Pembrokeshire (where its place in the series is so precisely marked in Broad Sound) we perceive *its ordi-*

* See small map of England, in the corner of the large map of the Silurian region. See also Phillips's General Geological Map of the British Isles. In the latter it has been inaccurately stated (in acknowledging the sources from which the author drew) that Devonshire is coloured from Mr. De la Beche. Professor Phillips has promised to correct this error in the new edition of his map which is about to appear, as he is fully aware, together with every geologist who was present at the meeting of the British Association, that we first proposed this great change: and even now, though Mr. De la Beche has followed us in separating the carbonaceous rocks from the greywacke, and represents them under a distinct colour, he does not admit them to be the equivalents of the carboniferous system of England.

nary lithological aspect almost entirely obliterated; the rocks which occupy the place of the soft mudstones and argillaceous limestones of Salop and Hereford, &c. have become hard, siliceous sandstones, with a slaty cleavage; and affording no evidences of clear subdivisions, they can be divided into broad groups by the help alone of *a very few* of the same fossils which teem in the great area of the Silurian region. As Pembrokeshire, lying as it does between the Silurian region and Devonshire, prepares us, by the peculiar structure of its coal or culm-field, to recognize an almost perfect analogy of structure in the culm-field of Devon; so do the great changes which the underlying old red and Silurian systems have there undergone, predispose us to believe that the strata which support the culm-field in Devon may present themselves also under a peculiar mineral aspect. Notwithstanding, however, this caution as to the possibility of some lower members of the Devonian and Cornish strata representing Silurian rocks, we adhere to the conviction, that the great mass of the strata which support and appear to pass upwards into the culm-field are the equivalents of the old red system properly so called.

Instead of thinking ourselves rash and hasty in making the generalization above given, we would rather accuse ourselves of being tardy and over-cautious; and we are now surprised (notwithstanding the imperfection of our evidence) that we so long retained the older rocks of Devon and Cornwall in the place where we classed them on our return from these counties in the autumn of 1836. For our conclusions, excepting their generality, are not entirely new. Some of the red sandstone groups, at least, of South Devon, have often been called *old red sandstone*; and they are so regarded by Mr. Austen; who also considers the Torbay limestone as the equivalent of the *mountain limestone.* Mr. Greenough, many years since, pointed out the extreme difficulty of separating the Plymouth limestone from the *mountain limestone*; and Mr. Lonsdale, a considerable time since, believed that the system of South Devon would at length be proved only a peculiar development of the *old red sandstone*; and he freely stated this opinion to Mr. De la Beche as well as to ourselves. The present opinions of Mr. De la Beche are before the public, and we have no right, perhaps, to be his interpreters. He puts forth several hypotheses, without positively adopting one of them. He must, however, (after the recent publication of such large groups of Silurian fossils) before long perceive that the formations of South Devon not merely contain fossils approaching those of the *mountain limestone* (a fact long known), but that their whole suite of fossils is intermediate between those

of the Silurian and Carboniferous Systems; a fact which at once defines their true place in the sequence of British rocks. The hypothesis he seems most inclined to adopt is the following:—That the calcareous and slaty system of South Devon is the newest, being above the carbonaceous system; and that the carbonaceous system is newer than the greywacke north of Barnstaple. In this way the calciferous band, "extending from Torbay, &c., into South Cornwall, would be in a higher part of the greywacke series, and might be even equivalent to the beds known as the old red sandstone."—(*Report*, &c. p. 149.)

We agree with the concluding remark; but not for the reason hypothetically stated, viz.: that South Devon is in a higher part of the greywacke series; for we place the North and South Devon groups on the same parallel, and consider the culm-measures as unequivocally superior to them both.

So long as we were unprovided with a typical suite of fossils from the older system of Devon, it was impossible to propose for it any name; but now, having discovered a great many of its fossils, and that too in regions wherein the red arenaceous character gives way to the slaty impress, and a very different mineral aspect; the necessity of adopting a new name becomes apparent, and we propose the term "Devonian System" as that of all the great intermediate deposits between the Silurian and Carboniferous Systems. The "Devonian System" is so far unexceptionable, that it may be applied, without any contradiction of terms, to rocks of every variety of mineral structure which contain the characteristic series of organic remains.

When these organic remains are described, we shall then have a regular descending order of the older fossiliferous strata in the three great lower systems which pass into each other, the *Carboniferous*, *Devonian*, and *Silurian*. Whether the still lower slaty rocks to which one of us applied the term *Cambrian*, may or may not contain, in their inferior parts, distinct typical fossils, is a problem not yet solved, though as far as our labours have gone, we know that many of the shells which characterize the lower Silurian group, exist also (even at considerable depths) in the great upper Cambrian group, and therefore the line we have provisionally drawn between the Silurian and Cambrian Systems may, eventually, be fixed by some natural grouping of fossils at a different level.

We proposed the use of the terms Silurian and Cambrian because we believed that their adoption, (L. & E. Phil. Mag., vol. vii. pp. 46, 483,) when applied to well-defined mineral masses, might tend to clear away the obscurity which we were per-

suaded would hang over the older rocks as long as they were all considered to belong to the dark and undefined æra of "Grauwacke;" and we trust that we have, in this memoir, shown strong additional grounds why this mineralogical term should be disused by geologists, as a term of classification, applied as it has been to rocks of such very different ages; thus serving as a shelter for ignorance, and paralyzing every effort for determining the succession of strata upon true principles. If its lovers wish still to cling to it, let them use it as an adjective and tell us of *Carboniferous*, *Devonian*, *Silurian*, and *Cambrian* "*Grauwacke*," and then, at least, the term will do no mischief. The continuance of the use of this term to represent different epochs in the history of the earth would be as absurd as to retain the old "flötz" formations of Werner, after it has been shown that such rocks are often as highly inclined as the most ancient strata; but we trust that any wrangling about this barbarous word is nearly at an end; for already some of the best foreign geologists have discarded its application to the upper systems of transition rocks, and now restrict its use as a term of classification to the lowest slaty or Cambrian rocks.

March 25, 1839.

[A Postscript to this paper will be found at p. 317 of the present Number.]

XLI. *On a very particular and curious Account of the Comet of* 1472, *from a contemporary MS. Chronicle in Peterhouse Library. By* J. O. Halliwell, *Esq.*, *F.S.A.*, *of Jesus College, Cambridge.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

The following minute description of the comet of 1472 is taken from an autograph chronicle of English affairs by John Warkworth, master of St. Peter's College, Cambridge, still preserved in the library of that institution. I am preparing the whole for publication for the Camden Society.

"And in same xi yere of the kynge, in the begynnynge of Januarii, there apperyd the moste mervelous blasynge sterre that hade bene seyn. It aroose in the Southe-Este at ij of the cloke at mydnyght, and so contynued a xij nyghtes, and it arose ester and ester till it aroose full este and rather. And so when it roose playn Est, it rose at x of cloke in the nyght, and kept his cours flamynge Westward overe Englond; and it hade a white flaume of fyre fervently brennynge, and it flamed endlonges fro the Est to the Weste, and noght vp-

right; and a grete hole theirin, whereof the flawme cam oute of. And after a vj or vjj dayes it aroose North-Est, and so bakkere and bakkere, and so enduryd a xiijj nyghtes full lytell chaungynge, goynge from the North-Este to the Weste; and sometyme it wuld seme a quenched oute, and sodanly it brent fervently ageyn. And then it was at one tyme playne northe, and then it compassed rounde aboute the lode sterre, for in the evynynge the blase went ageyns the Southe. And in the mornynge playne northe, and then afterward West, and so more West flamyng vpryght, and so the sterre continued iiij wekys tylle the xx day of ffeveryere; and when it appered West in the fyrmament, then it lasted alle the nyght, somewhat discendyng with a grettere smoke on the heyre; and som men seyd that the blassynges of the seide sterre was of a myle lengh; and a xjj dayes afore the vanyschynge therof, it appereryd in the evenyng, and was down anon within two oures, and evyr of a colour pale stedfast, and it kept his course rysynge west in the northe, and so every nyght it apperid lasse and lasse tyll it was as lytell as a hesyll styke, and so at the laste it vanesched away the xx day of ffebruarii. And some men saide that this sterre was seen ii or iij oures afore the sunne rysynge in Decembre iijj days before Chrystynmasse in the Southwest, so by that reasoune it compassed rounde abowte alle the erthe alleway chaungyng his cours as is afore rehersid."

The observations of Johannes Regiomontanus upon the same comet are recorded in the Nuremberg Chronicle. Warkworth's description was lately commuicated to the Society of Antiquaries by my friend Mr. Bruce, and my object in sending it to you was to afford an opportunity for those of your readers to peruse it who are not likely to meet with it through any other source. Your obedient servant,

J. O. Halliwell.

XLII. *Observations on some points in the Theory of the Dispersion of Light. By the Rev.* Baden Powell, *M.A., F.R.S., F.G.S., F.R.A.S., Savillian Professor of Geometry, Oxford.**

THE problem of the *dispersion* was, for a long time, confessedly the opprobrium of all theories of light, but more especially, in proportion to its higher pretensions,—of the undulatory, not simply because it *had not explained* those phænomena, but because, according to the received views, it was *at variance* with them. It has only been within the last few years, that, by a modification of the principles of this

* Communicated by the Author.

theory, it has been brought to bear on the question of the dispersion. Yet there are still those who are so unreasonable as to inveigh against the theory, because it has not, in that comparatively short period, succeeded in clearing the subject of all its difficulties; and who, instead of any degree of satisfaction at what *has* been done, express only displeasure at what *has not* been effected. Passages might be cited (were it to any purpose to do so) from the writings of some philosophers of the present day, in which complaints of this nature occur, involving remarks and reflections which we cannot well set down to ignorance, and which, in any other point of view, can neither be regarded as peculiarly creditable to the taste of the writers, nor to their appreciation of what is justly due to a series of researches of such a character as must fairly be allowed to belong to those in question, even if they could be shown to have failed in their object.

It is not however my design in the present observations to enter into controversy, but merely to offer a few remarks on one or two points connected with that part of the theory which at present seems most open to difficulty and objection; and to which the attention of those who are in a condition to grapple with the difficulties of this intricate portion of dynamical research, is at present most powerfully called.

In my first deductions from the theory of M. Cauchy, and my earliest calculations to compare it with observation, I made use of a formula which was allowed all along to be but an imperfect and approximate one, and which was applied numerically only by the aid of an indirect and tentative process. A *direct* mode of calculation by it, was indeed stated and explained (under a certain material limitation) (see London and Edinburgh Journal of Science, April 1836, vol. viii. p. 309), but this was in practice not less troublesome than the former.

The other and more exact methods proposed by Sir W. R. Hamilton and Mr. Kelland, by which my later computations were carried on, involve a greater number of constant coefficients; which by consequence, directly or indirectly, must be assumed from observation. The nature of these assumptions, and the precise difference between the two formulas, I pointed out in a paper in the Lond. and Edinb. Journal of Science, January 1836, (p. 26,) and March 1836, (p. 206). It will be desirable here briefly to restate them:

The exact formula deduced immediately from M. Cauchy's theory is,

$$\frac{1}{\mu^2} = \Sigma\left\{ H^2 \left(\frac{\sin^2\left(\frac{\pi \Delta x}{\lambda}\right)}{\left(\frac{\pi \Delta x}{\lambda}\right)^2} \right) \right\} \qquad (1.)$$

This, it may be well to observe, is equivalent to the sum

of a number of like terms, for the same value of μ and λ, such as,

$$\frac{1}{\mu^2} = \left\{ \begin{array}{l} \mathrm{H}_{\prime}^{2} \left(\dfrac{\sin^2\left(\dfrac{\pi \Delta x_{\prime}}{\lambda}\right)}{\left(\dfrac{\pi \Delta x_{\prime}}{\lambda}\right)^2} \right) \\ + \mathrm{H}_{\prime\prime}^{2} \left(\dfrac{\sin^2\left(\dfrac{\pi \Delta x_{\prime\prime}}{\lambda}\right)}{\left(\dfrac{\pi \Delta x_{\prime\prime}}{\lambda}\right)^2} \right) \\ + \mathrm{H}_{\prime\prime\prime}^{2} \left(\dfrac{\sin^2\left(\dfrac{\pi \Delta x_{\prime\prime\prime}}{\lambda}\right)}{\left(\dfrac{\pi \Delta x_{\prime\prime\prime}}{\lambda}\right)^2} \right) \\ + \text{\&c.} \end{array} \right. \qquad (2.)$$

The summation being extended to all the values of Δx which it may be necessary to include.

My first and approximate attempt at calculation was effected by taking a *single term* of such a series, with some constant coefficient, which might be a sort of mean among all the similar terms, and which (on extracting the root) would be,

$$\frac{1}{\mu} = \mathrm{H} \left(\frac{\sin\left(\dfrac{\pi \Delta x}{\lambda}\right)}{\left(\dfrac{\pi \Delta x}{\lambda}\right)} \right) \qquad (3.)$$

The peculiar function involved is easily expanded into a series; and in this way the *exact formula* (1.) may be expressed by,

$$\frac{1}{\mu^2} = \mathrm{P} - \mathrm{Q}\frac{\mu^2}{\lambda^2} + \mathrm{L}\frac{\mu^4}{\lambda^4} - \text{\&c.} \qquad (4.)$$

Supposing 3 terms sufficient, this is the formula of Mr. Kelland: in which the coefficients can only be found by *assuming 3 indices from observation.* An objection has also been urged by Professor Lloyd, that, on pursuing the investigation, from the relations which subsist between these constants, values of them are given by calculation which are at variance with those derived from observation. I have also remarked in referring to this point at the end of my paper (Phil. Trans. 1838, part i.) that to propose as a sufficient theory one which requires so large an assumption of data from observation, appears hardly consistent with the just demands of what ought to constitute a legitimate mathematical explanation of the law of experimental results.

Now to all who have attentively considered the nature of those remarkable optical properties of bodies which are called their *refractive* and *dispersive powers*, it will be evident that we have *a very peculiar* case to consider. The problem we have to solve is rather *a combination of two distinct problems.* The dispersive and refractive powers follow no proportion to each other, and it is almost impossible to conceive any theory, or even any empirical mathematical law, which could connect the two together. For example, we have diamond and water, with refractions nearly double the one of the other, and dispersions nearly the same. Flint glass and oil of cassia with the same refractions nearly, and dispersions as about 1 to 3. In a word, the absolute magnitude of the deviation of any given ray, or of white light, bears no relation whatever to the difference of deviation between the extreme rays of the spectrum, in different media. If then we seek a theory to explain the facts, it would be not only unreasonable to expect it to connect such obviously incongruous phænomena, but it ought most rationally to involve *two distinct constants*, one belonging to the *refractive* POWER, the other to the *refractive* CHARACTER of the medium. And in conformity with the general conditions of formulas of this kind, we might expect that, directly or indirectly, we should have to assume *two* quantities as given by observation, in any calculation to compare theory with observation, for the spectrum of a particular medium.

Now if we take the formula (1.) above stated, it is at once manifest that if Δx be very small compared with λ, we have for all values of λ very nearly,

$$\frac{\sin\left(\frac{\pi\,\Delta\,x}{\lambda}\right)}{\left(\frac{\pi\,\Delta\,x}{\lambda}\right)} = 1 \qquad (5.)$$

or there is no dispersion, and the formula is reduced to

$$\frac{1}{\mu^2} = \Sigma\,.\,\mathrm{H}^2 \qquad (6.)$$

Now, in any medium, if Δx be *not* very small compared with λ, we shall have different refractive indices for each ray, which will differ less as λ becomes greater; and if we suppose λ to increase indefinitely, ($\Delta\,x$ remaining finite) the above expression (6.) will be the *limit* to which the refractive index constantly tends, and from which it does not sensibly differ when λ is supposed large; that is, for some ray beyond the red end of the spectrum (and such may exist though not sensible to the eye) there is an absolute limit to all refraction,

different in different media; this may be called *the refraction constant*. If we had any means of determining it by experiment, it would be a most important step for the theory. But it is evidently a datum quite independent of the dispersion. This quantity corresponds to (h) in Mr. Tovey's notation, and to (P) in Mr. Kelland's, who has determined it for Fraunhofer's media; as I have done for several of those I have examined. (Phil. Trans. 1838, part i.) It is an index not greatly below that for the red extremity of the visible spectrum. Whether it may *possibly* be connected with the refraction of *heat* may become an interesting question.

To return to the dispersion:—according to the exact method we have still two more constants to determine, viz. in Mr. Kelland's notation (Q) and (L); in Mr. Tovey's (h') and (h''): these like the former must be ultimately derived from observation, and they are in fact obtained from the assumption of *two more* observed indices.

But pursuing the same idea as that just adverted to, it would seem reasonable that as *one* datum for each medium is the *refraction constant*, so *one* more should supply a *dispersion constant*; and, on looking at the nature of the formula, it would seem that this should depend on the arc $\pi \Delta x$. Now the coefficients Q and L arise from the summation of the various combinations of $H_{\prime}$ and $\Delta x_{\prime}$; $H_{\prime\prime}$ and $\Delta x_{\prime\prime}$; $H_{\prime\prime\prime}$ and $\Delta x_{\prime\prime\prime}$, &c. And if we recur to the simple consideration of one term of the summation, which furnished my first approximate formula, we shall see that we here avoid this difficulty, and in fact have only the *two* constants of refraction and dispersion to derive from observation. And though it may be true in reference to the physical theory that for the adoption of that formula no legitimate ground appears, yet for the reasons already adduced it may be worth while to examine the application of it more closely.

If we take the simple formula (3.), or what is the same thing, on developing, (see Lond. and Edinb. Journal of Science, Jan. 1836) and substituting the value of λ in air,

$$\frac{1}{\mu} = \mathrm{H}\left\{1 - \frac{1}{6}\,\frac{\mu^2}{\lambda^2}\,(\pi\,\Delta\,x)^2 + \frac{1}{120}\,\frac{\mu^4}{\lambda^4}\,(\pi\,\Delta\,x)^4\ \&\text{c.}\right\} \quad (7.)$$

writing for abridgement, $\pi \Delta x = \theta$,

$$p = \frac{1}{6}\,\frac{\mu^3}{\lambda^2},\ \text{and}\quad q = \frac{1}{120}\,\frac{\mu^5}{\lambda^4},$$

we shall have, for any one ray,

$$\frac{1}{\mathrm{H}} = \mu - p\,\theta^2 + q\,\theta^4 \quad (8.)$$

and for any other ray of the spectrum,

$$\frac{1}{H} = \mu_{,} - p_{,} \theta^2 + q_{,} \theta^4 \tag{9.}$$

Then eliminating H, we have

$$0 = (\mu - \mu_{,}) - (p - p_{,}) \theta^2 + (q - q_{,}) \theta^4 \tag{10.}$$

and

$$\frac{-(\mu - \mu_{,})}{(q - q_{,})} = \theta^4 - \left(\frac{p - p_{,}}{q - q_{,}}\right) \theta^2 \tag{11.}$$

or for abridgement

$$-m = \theta^4 - r\theta^2 \tag{12.}$$

whence by solving the quadratic we obtain

$$\theta^2 = \frac{r}{2} \pm \sqrt{-m + \frac{r^2}{4}} \tag{13.}$$

in which

$$\frac{r}{2} = 10 \frac{\left(\frac{\mu^3}{\lambda^2} - \frac{\mu_{,}^3}{\lambda_{,}^2}\right)}{\left(\frac{\mu^5}{\lambda^4} - \frac{\mu_{,}^5}{\lambda_{,}^4}\right)} \tag{14.}$$

$$m = \frac{(\mu - \mu_{,})}{\frac{1}{120}\left(\frac{\mu^5}{\lambda^4} - \frac{\mu_{,}^5}{\lambda_{,}^4}\right)} \tag{15.}$$

If then we take the *two* values μ and $\mu_{,}$ from observation, we readily find θ, and thence again obtain the value of H, by substituting in either of the expressions (8.) (9.). Then, in the same expression for every other ray of the spectrum, substituting θ, we shall find H; which, if the formula be correct, ought to result the *same* for every ray.

This process then, if it be considered allowable, affords this material advantage, that it requires only the assumption of *two* values as given by observation for each medium, which we have just seen is exactly the improvement required in theory. Now though I have not as yet tried the result of calculation precisely in the way above stated, yet it is evident that all the calculations I made in my two first papers in the Phil. Trans., including all the results of Fraunhofer and Rudberg, were conducted on an hypothesis which is in principle identically the same: and in all those it is universally allowed the coincidences are as close as could be desired: so that it is evident that this supposition cannot be very far from the truth; calculation grounded upon it has not yet been applied to more highly dispersive media. But it becomes extremely important to see whether it may be so applicable; whether it may apply even *as well as* the methods proposed on the less restricted hypotheses, such as I have em-

ployed in my later calculations; but which essentially involve the fault of requiring too large assumptions from observations, and these too, according to the remarks of Prof. Lloyd, leading to conclusions inconsistent with truth: this he has pointed out in a letter to myself; the details of his investigation have not yet been published; but there is a short abstract of them in the Proceedings of the Royal Irish Academy, No. 1. p. 10, and some mention of the subject was, I believe, made at the British Association 1838, in the Physical Section. I have reason to hope that the paper will before long be published, meanwhile it may not be irrelevant to offer one observation more on the subject.

Since such a formula as the above is unquestionably very nearly accordant with the truth, through, at least, a considerable range of media, it certainly becomes important to examine, whether any *theoretical* conditions are conceivable, which may warrant the adoption of it, as at least a good approximation; and the question obviously reduces itself to this: whether in taking the sum of an indefinite number of terms which are combinations of $H_{\prime}$ and $\Delta x_{\prime}$, $H_{\prime\prime}$ and $\Delta x_{\prime\prime}$, $H_{\prime\prime\prime}$ and $\Delta x_{\prime\prime\prime}$, &c. (the form of which is seen in my paper, Phil. Trans. 1838, part ii.), we may not discover some ground for justifying the adoption of *a single term* containing some mean value of Δx combined with a constant coefficient H, such, that we shall have accurately or nearly,

$$\left\{\begin{array}{l} \left\{H_{\prime}^{2}\dfrac{\sin^2\left(\frac{\pi\Delta x_{\prime}}{\lambda}\right)}{\left(\frac{\pi\Delta x_{\prime}}{\lambda}\right)^2}\right\} \\ +\left\{H_{\prime\prime}^{2}\dfrac{\sin^2\left(\frac{\pi\Delta x_{\prime\prime}}{\lambda}\right)}{\left(\frac{\pi\Delta x_{\prime\prime}}{\lambda}\right)^2}\right\} \\ +\left\{H_{\prime\prime\prime}^{2}\dfrac{\sin^2\left(\frac{\pi\Delta x_{\prime\prime\prime}}{\lambda}\right)}{\left(\frac{\pi\Delta x_{\prime\prime\prime}}{\lambda}\right)^2}\right\} \\ +\,\&c. \end{array}\right\} = H^2\,\frac{\sin^2\left(\frac{\pi\Delta x}{\lambda}\right)}{\left(\frac{\pi\Delta x}{\lambda}\right)^2}$$

How far the dynamical conditions of a system of molecules such as that supposed by M. Cauchy and the other eminent mathematicians who have treated on the subject, may be found susceptible of leading to any such deductions (in which the process would be perhaps somewhat analogous to finding the expression for the centre of gravity of a system of particles,)

I would leave to the meditations of those more familiar with the analysis of dynamics. But I trust some of those readers of this Journal, who are so able to discuss such a topic will not think it unworthy of their attention.

XLIII. *Meteorological Observations made during Voyages in the Atlantic and South Pacific Oceans; and Altitudes in the Vicinity of Lima measured by the Sympiesometer.* By John Maclean*.

Metereological Register from the Cape de Verd Islands to Callao. Thermometer in the Cabin, and a fair exposure.

Date.	Thermometer 8 a.m.	Noon.	Latitude.	Wind and Weather.	Observations.
1821.					
Sept. 22.		85		calm and clear.	off Cape Arit.
23.		82		light air.	Cape de Verd.
30.		83			
Oct. 1.		83	7° 3′	N. lat.	
2.	76	78†	4 59	WSW., rainy.	long. 17° 30′
3.		82‡	4 44	S., clear.	
4.		82§	4 4	S., clear.	
5.		82	3 34	southerly.	
6.		81	3 4	"	
7.		81	2 46	"	
8.	79	81	2 25	SSW.	
9.	78	81	1 47	SSW., breezes.	
10.	78	79	1 37	SSW.	long. 14°
12.	78	80	1 0	S. by E.	
13.	78	79	0 20	S. lat.	
14.	78	80	1 25	S. by W., light.	
15.	77	79	2 42	SE. by E., breezes.	
16.	77	79	4 18	cloudy.	
17.	77	80	6 32	"	
18.	76	79	8 48	"	
19.	76	79	10 47	SE. by E.	
20.	76	79	12 43	"	
21.	76	79	14 55	"	
22.	76	79	16 48	NE.	many birds.
23.	77	80	18 38	N. by E.	
24.	77	80	20 40	variable, rainy.	
25.	74	74	21 13	S.	
26.	74	73	22 21	ESE., cloudy.	

* Communicated by Professor Sir W. J. Hooker, LL.D., F.R.S. The author observes, with respect to his Meteorological Observations: "My chief object in sending these is to show the trifling change in the thermometer during winter and summer in the extreme southern latitude, where it appears the cold is only severe when the wind is from the southward."

† At 4 p.m. 79°. ‡ At 6 a.m. 80°; 8 p.m. 82°. § At 6 a.m. 79°.

TABLE *continued.*

Date.	Thermometer. 8 a.m.	Thermometer. Noon.	Latitude.	Wind and Weather.	Observations.
1821.					
Oct. 27.	73	73	23° 29′	rainy.	seldom birds after noon.
28.	70	72		clear.	
29. to Nov. 22.		80 to 88	at	Rio Jan.	
23.	76	78	25 29	NE., clear.	
24.	76	78	26 55	N., cloudy.	
25.	76	76	27 46	light winds.	
26.	70	70	29 20	SE.	
27.	67	68	31 18	ESE., cloudy.	Many birds.
28.	70	72	33 24	NE., clear.	
29.	70	72	35 32	light clouds.	
30.	61	62	36 27	cloudy.	Many seals and penguins.
Dec. 1.	54	56	36 34	S., clear.	Always birds. Sea and clouds have a strange colour; generally whales in the morning early and but few whales, and no seals, seen after 40° and 45° S. lat.
2.	59	61	37 32	NW., cloudy.	
3.	58	58	39 3	SE., blew fresh.	
4.	55	55	39 46	SE., clear.	
5.	57	57	41 46	N., cloudy.	
6.	50	52	43 17	southerly, clear.	
7.	55	52	45 25	W. by SSW., blowing.	
8.	53	51	46 53		
9.	48	49	48 14	W. by S. clear.	
10.	49	49	49 46	NW. & W., cloudy.	
11.	49	49	51 41	W. & WSW., clear.	Long. 59° 4′
12.	47	49	52 31	N. NW. & SW., rainy, clear.	
13.	42	44	53 1	SW. gales, & hail-storms, but generally clear.	
14.	41	42		S. by W., gales, with snow and hail, cloudy.	
15.	46	48	53 2	W., changeable, and towards evening calm.	Long. 58°.
16.	48	50		SSE., calm, cloudy.	
17.	42	41	53 42	" "	
18.	47	47	54 36	blowing, and changeable, and hail.	Immense numbers of ducks from Staten Land.
19.	43	43	54 57	SE. to WSW., clear and cloudy.	
20.	42	43	56 25	WSW., to WNW., hazy.	
21.	47	48		WNW., foggy and rain.	
22.	47	49	57 22	NW., do.	
23.	45	47	57 33	hazy with rain, then clear.	

Table *continued.*

Date.	Thermometer. 8 a.m.	Noon.	Latitude.	Wind and Weather.	Observations.
1821. Dec. 24.	45	45	57 43	NW. to WSW., blowing hard gales.	Always birds except in very bad weather, but on the 24th immense numbers of sea birds and pardelas.
25.	42	43	58 17	WSW. to W., gales and clear.	Read a book at midnight by daylight.
26.	42	42	58 45	SW. by W., cloudy and clear.	Long. 71 .
27.	44	46	58 23	W. by S. to SW. by W., foggy & rain.	
28.	44	44		WNW. to WSW., foggy then clear.	
29.	42	41	59 54	WSW. to SW., changeable weather in the morning.	
30.		42		NW. by N. to E., thick rainy.	Lat. 60° 3′.
31.		46		W. by N. to W. by S. hard gales with hail.	
1822. Jan. 1.		44	57 56	SW. to WNW., clear then cloudy. Long. 75° 31.	Sometime past north-easterly currents; many sea birds, few pardelas, some penguins, and until to-day no albatroses.
2.		46	57 48	WNW. to W., cloudy.	
3.		46	58 36	NW. by N. to N. Long. 77° 30′.	
4.		47	58 31	NW., clear. Long. 79° 50′.	Grampuses partly white, and porpoises black and white.
5.		46	58 50	NW., hazy. Long. 82° 31′.	
6.		44	58 23	SW., clear, then cloudy. Lon. 83°.	
7.		49		SW., thick, with rain.	Many *Molly Manks* and other birds, apparently of passage, smaller than pigeons: much sea-weed: no pardelas for several days: few sea birds; occasionally an albatros.
8.		50	55 4	NW., hazy & rain. Long. 83° 13′.	
9.		49	55 5	NW.; clear, hail, and rain in the night.	
10.		50	54 46	NE., clear & calm.	
11.		49	54 4	SW., hazy & calm.	
12.		50	52 52	SW. to NW. cloudy and clear.	A flock of small apparently land birds.
13.		51	52 10	NW. to SW., cloudy and rain.	A large whale. Variety of birds and two divers.
14.		51	49 59	SW., cloudy.	
15.		52	47 6	SE., clear & clou.	Grampuses; albatroses, always a few.

TABLE *continued.*

Date.	Thermometer.	Latitude.	Wind and Weather.	Observations.
1822.				
Jan. 16.	54	45 19	W. by N., cloudy and clear. Long. 83° 42′.	
17.	59	43 8	„ „ Long. 82°.	
18.	64	41 33	NW. to SW., cloudy and clear.	
19.	63	40 32	SE. Long. 79° 30′.	Few or no birds these
20.	64	38 40	ESE., cloudy.	four days.
21.	64	36 43	SE., cloudy and very clear.	In apparent soundings for several hours:
22.	64		Long. 75° 30′	Query, Ulloas shoals?
23.	65			Arrived at Valp.
Mar. 1.	65		WSW., cloudy.	
2.	65	32 30	calm and cloudy.	
3.	66	31 32	SW. to S., cloudy.	Many whales.
4.	68	29 46	SW., „	
5.	68	28 17	S., „	
6.	70	24 48	SSE., cloudy & clear.	
7.	71	25 38	SSE., cloudy.	
8.	72	24 4	cloudy and clear.	
9.	72	22 15	SSE. to E., cloudy.	
10.	74	20 2	E. by NE., cloudy and clear, some rain early in the morning.	
11.	76	17 42	E. by NE., clear and cloudy.	
12.	77	15 27	E. to SSE., clear and cloudy.	Several sperm whales.
13.	71	13 50	SSE. to S. by W., clear.	Many and various birds, weed and seals.
14.	75	12 15	SSE., cloudy.	Whales and many birds.

Meteorological Register round Cape Horn beyond the Equator. Thermometer on Deck.

Date.	Thermometer. 8 a.m.	Latitude. South.	Wind and Weather.	Observations.
1833.				
May 8.	55°	37 46	W., breezes.	
9.	54	40 46	„ „	
10.	„		calm.	Caught many pintados.
11.	49	44 4	SW., light winds.	
12.	50	no obs.	SE., stiff breeze.	
13.	46	„	ditto.	
14.	45	„	NE., fine breeze.	

TABLE *continued.*

Date.	Thermometer. 8 a.m.	Latitude. South.	Wind and Weather.	Observations.
1833.				
May 15.	45	no.obs.	NE., fine breeze.	Accompanied generally by petrels; a few sea birds, an occasional albatros.
16.	43	54 14	NW., fine breeze and weather.	
17.	44		„ cloudy.	
18.	43		N., thick and squally.	
19.	43	56 37	NW., stiff breeze. Long. 70° W.	
20.	42		NW., hard gale.	
21.	41	57 28	NW., snow in the morning.	
22.	41		clear weather.	Passed Diego Ramirez.
23.	39	56 3	WNW., fresh breeze and clear.	
24.	42	55 37	NW., gales of wind and clear.	
25.	40		NW., sleet or fine snow.	
26.	40	53 52	NW., hard gales and clear.	
27.	37	51 48	W., clear.	
28.	37	49 50	SW., clear then cloudy.	
29.	35		S., snow during this day.	
30.	37	45 15	SE., clear and cold.	
31.	40	42 35	S. clear and pleasant.	
June 1.	48	40 5	SW., ditto.	
2.	52	38 52	NW., ditto.	A whale.
3.	54	37 48	nearly calm.	
4.	59	35 40	SE., gales.	
5.	59	32 54	SE., breeze, cloudy.	
6.	60		EN., gales, dark rainy weather.	
7.	72	28 15	NE., gales, ditto.	Birds left us.
8.	70	27 37	NW., night tremendous squall with lightning, morning cloudy, and day fine.	A few birds returned.
9.	75	25 41	NW., cloudy, then clear.	
10.	73	25 5	W. E., clear and changeable.	
11.	75	24 37	NNW. to NNE.	No birds now with us.
12.	72	23 50	variable.	
13.	75	22 18	E., breeze, the trade wind.	
14.	77	19 42	E.	Passed the Island of Trinidad.
15.	78	16 23	E., fine breeze.	
16.	80	13 16	E.	Flying fish, falling stars.

TABLE *continued.*

Date.	Thermometer 8 a.m.	Latitude. South.	Wind and Weather.	Observations.
1833.				
June 17.	80	10 13	E., fine breeze.	
18.	81	7 6	E. by S. „	
19.	81	3 51	„ „	
20.	82	1 2	S. ESE. „	Many Physalia pelagica.
21.	81	1 30	N. SSE., cloudy.	
22.	82	2 53	S., light wind and clear.	
23.	82	3 42	„ „ „	
24.	80	5 17	„ „ „	
25.	80		W. and NW. „	
26.	80	7 56	NE. „	A shoal of porpesses, emitted an unpleasant smell.
Noon	89			
27.	80		calm and heavy rain.	
28.	80	9 7	NE., fine breeze.	
29.	81	10 9	NNE., clear weather.	
30.	81	12 10	NE., fresh breeze, cloudy.	
July 1.	78	14 22	„ „ „	
2.	79	16 24	„ „ Long. 37°.	
3.	78	18 57	„ „ „	
4.	76	21 34	NE. by E. „	
5.	79	24 27	ENE., clear weather.	
6.	79	27 9	„ „	
7.	79	28 25	„ „ light wind.	
8.	79	29 17	E. „ „	
9.	80	30 14	„ „ „	
10.	80	30 56	NNE., nearly calm; much gulf wind; snow some days.	

Meteorological Register from the Canary Islands to the Port of Callao. Thermometer chiefly if not always on Deck.

Date.	Thermometer 8 a.m.	Latitude.	Wind and Weather.
1834.			
April 25.	67	29° 43′	WNW., clear.
26.	69	28 40	„ light and clear.
27.	70	27 16	N., breeze. „
28.	70	25 5	NE. „ „
29.	72	22 44	„ „ „
30.	72	19 58	„ „ „
May 1.	71	17 24	„ „ „
2.	73	15 20	„ „ „
3.	73	12 54	„ „ „ partial clouds.

TABLE *continued.*

Date.	Thermometer at 8 a.m.	Latitude.	Wind and Weather.
1834.			
May 4.	73	10 46	NE., clear at sunrise; thermometer at $71\frac{1}{2}°$, noon 77°.
5.	75	8 53	NE., but light: thermometer sunrise 73°, in sun at noon 102°, in shade 77°, sunset 77°.
6.	79	7 1	,, clear.
7.	81	5 33	,, afternoon variable and rain: thermo. in the rain 75°, surface of the sea 82°.
8.	78	4 46	variable with rain.
9.	80	4 9	NW., squally and heavy rain.
10.	83		dead calm.
11.	84	3 30	SE., light. Long. 23° 35′.
12.	82		calm, or light variable winds.
19.	83	1 41 N.	Therm. at noon 85°
20.	83	19 S.	SE., fine breeze and clear. Long. 26° 40′.
21.	82	1 30	,, ,, thermometer noon 87°.
22.	83	3 31	,, ,, ,, do. 85°.
23.	81	5 15	,, morning squally, rain. Long. 27° 35′.
24.	82	7 36	,, fresh breeze and clear: therm. in shade at noon 85°, do. sun 106°.
25.	81	9 50	,, morning cloudy with light showers; shade at noon 83°, sun 104°.
26.	81	12 17	,, fine breeze and clear; extremely hot some time past, today somewhat cooler.
27.	80	14 41	,, fine trade wind.
28.	79	17 6	SE., trade still.
29.	78	18 47	light, cloudy, then calm.
30.	78	20 20	light, and varied from NW. to SW. with rain.
31.	76		SE., cloudy and rain.
June 1.	76	22 14	,, light winds.
2.	76	22 58	,, ,, ,, and clear.
3.	73	23 20	,, ,, squally last night and this morning, then clear: therm. air 7 a.m. 72°, do. water 78°; afterwards descended to 67°, from evaporation of course.
4.	71	24 28	,, cloudy.
5.	73	25 38	,, light wind, then calm.
6.	73		NE. to WNW. and light.
7.	73	27 52	NE., fine breeze.
8.	72	30 37	NNW. then W., fresh breeze; thermometer at sunset 67°.
9.	62	32 37	W., blowing a gale.
10.	59	33 10	WSW., ditto.
11.	58		S., light, thermometer in sea 64°.
12.	60	35 5	NW., light.
13.	61	36 47	WSW., fine breeze and clear.
14.	60	38 28	WNW., fine breeze, heavy dew in the night, though cloudy.

TABLE *continued.*

Date.	Thermometer at 8 a.m.	Latitude.	Wind and Weather.
1834.			
June 15.	57	39 34	SE., light and drizzling thick weather.
16.	53	40 30	,, blowing fresh and dark, then SW.
17.	54	40 10	SW., ,, hard. Long. 52° 50′.
18.	50	40 0	,, wind more moderate, small rain; thermometer in rain 46°.
19.	45	40 36	,, blowing hard: thermometer noon 48°.
20.	44	41 13	nearly calm, and thick afternoon, clear; thermometer in sun at noon 60°, shade 53°, water 49°, after evaporation 47°.
21.	48	43 30	E., fine breeze; total eclipse of moon from 1 until 7 a.m., very clear atmosphere.
22.	47	46 6	NW., blowing fresh; thick morning, cleared towards noon.
23.	44	48 48	NNW., a gale, thick weather.
24.	47	50 55	NW., more moderate, thick; thermometer at sunset 45°.
25.	44	53 15	WSW. to N. by E., light rain, afterwards clear.
26.	44	54 10	light wind; descried Staten Land; thermometer in sun at noon 55°, shade at noon 45°, at sunset 43°, 8 p.m. wind of the snow covered land 40°, in water 44°: many luminous Infusoria caught here, as well as in other parts near the line.
27.	44	55 2	NW., light; thermometer noon 46°.
28.	42	56 2	NE., ,, hazy weather.
29.	44		NW., ,, ,, and light rain: made the island of Diego Ramirez.
30.	40	57 22	NW., blowing hard, thick weather; thermometer at noon 41°.
July 1.	38	58 4	W. in night, morning SW. clear.
2.	37	56 38	SSW., fine breeze, and clear; thermometer at noon in sun 38°, in water 42°.
3.	36	55 14	calm, then a breeze from SSE. with hail, sleet, and snow; thermometer at noon in sun 40°.
4.	40	52 28	SSE., fresh breeze, hail and snow.
5.	41	49 45	SW., fresh breeze; therm. shade at noon 42°.
6.	46	47 32	NW., ,, ,, and thick; therm. shade at noon 48°.
7.	46	45 46	,, moderate ,, ,, ,, 47°.
8.	50	45 47	,, ,, ,, evening NE.
9.	51	45 2	N. by W., to N. by E. and squally at noon.
10.	48	44 20	W., blew hard; thermometer at noon 48°.
11.	50	43 2	night fell calm, then N. by E. wind.
12.	53	42 24	NW., thick weather.
13.	50	41 2	,, variable, squally.
14.	51	38 28	SW. and W., pretty clear weather.
15.	55	36 28	NW., fresh and thick.
16.	54	34 11	SW. morning, then calm clear weather.
17.	55	33 13	,, clear weather; thermometer at noon 66° in the sun.

Table *continued.*

Date.	Thermometer 8 a.m.	Latitude.	Wind and Weather.
1834.			
July 18.	58	31 40	sw., light and clear, heavy dew in night.
19.	58	29 30	„ heavy dew and thick fog in morning.
20.	61	27 17	„ light wind.
21.	61	25 17	s., breeze, morning cloudy, noon cleared.
22.	61	22 45	ese., fresh and cloudy.
23.	61	19 26	se., fresh and cloudy; therm. sunrise 59°.
24.	61	16 19	ese., fresh and cloudy.
25.			got near Callao.

Heights of Places in the Neighbourhood of Lima, observed by the Sympiesometer.

Date.	Names of Places.	Hour of Observation.	Temperature.	Height in English feet.	Hygrometer. Boiling point of water.	Leagues distance.	
1837.							
Dec. 22.	Lima to say.........			500			
„	Cavalluo	10 a.m.	81°	1,166	15° 208′	6	6
„	Hill at Rio Seco ...	2 p.m.	78	4,704		3	9
23.	Santa Rora de Quiveo	5 a.m.	59	3,700	20 204½	5	14
„	Jaso	10 „	70	5,143	22 202	4	18
24.	Obragillo..............	5 „	47	8,944	9	5	23
„	San Buenaventura ..	9 „	59	8,969	13	2½	25½
„	Cross above S. Jose...	noon.	60	9,998		2½	28
25.	Huamautanga.........	8 a.m.	49	11,270	17 192	2½	30½
26.	Pumchucu	5 „	51½	8,630	18 195	2½	
„	Cruz Verde	9 „	69	4,655		3	5½
„	Macao	noon.	83	2,248	28	4	9½
„	Pumchanca	4 p.m.	75	853	21 209	4	13½
27.	Lima			500	14 210	5	18½

N.B. The hygrometer denotes the degree of cold necessary to produce the dew-point.

XLIV. *The Bakerian Lecture.—On the Theory of the Astronomical Refractions.* By James Ivory, *K.H., M.A., F.R.S. L. & E., Instit. Reg. Sc. Paris, Corresp. et Reg. Sc. Gottin. Corresp.**

THE apparent displacement of the stars caused by the inflection of light in its passage through the atmosphere, is treated by the astronomer like most other irregularities which he has occasion to consider. A set of mean quantities

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is first provided; and the occasional deviations of the true places from the mean are ascertained and corrected according to the state of the air, as indicated by the meteorological instruments. The subject of the astronomical refractions is thus resolved into two parts very distinct from one another; the first embracing the mean refractions, which are an unchangeable set of numbers, at least at every particular observatory; the second relating to the temporary variations occasioned by the fluctuations which are incessantly taking place in the condition of the atmosphere. It is the first of these two questions chiefly, or that regarding the mean refractions, of which it is proposed to treat in this paper.

In order to form a just notion of the mean refractions, we may suppose that some particular star is selected, and assiduously observed for a course of time so considerable as to comprehend every possible change in the condition of the atmosphere; all these observed places being severally reduced to some assumed state of the thermometer and barometer, and being combined so as to eliminate occasional irregularities, will determine the mean refraction of the star. In this procedure it is supposed, what experience confirms, that the result will ultimately be the same for the same altitude above the horizon, provided the observations are numerous enough, and extend over a sufficient length of time. We may instance the star α Lyræ observed by Dr. Brinkley; his observations are forty-four in number, extending over five years; and the greatest deviation of single observations from the mean quantity may be stated at $\pm 20''$. The supplementary table, extending from $85°$ to $89°\frac{1}{2}$ of zenith distance, published in Bessel's *Tabulæ Regiomontanæ*, is one of mean refractions calculated from many observations at every altitude. The table, in the same work, extending to $85°$ of distance from the zenith, which the supplementary one is intended to complete, may likewise be considered as having the authority of actual observation; for although a theoretical formula was used in the calculations, yet the results have been carefully corrected by a comparison both with the observations of Bradley and with those made with very perfect instruments in the observatory over which Bessel presides. These two make together a table of mean refractions of the highest authority; and being free from hypothetical admissions, to speak with precision, they form the only table of the kind of which astronomy in its actual state can boast.

The mean refractions, being a fixed set of numbers at any proposed observatory, are independent of temporary changes in the state of the air. If the general constitution of the at-

mosphere were so well known as to enable us to deduce the temperature, the density, and the pressure at any given altitude, from the observed condition of the air at the earth's surface, it might be possible to pitch upon an atmosphere intermediate between the extreme cases, in which the irregularities would compensate one another. From such an atmosphere the mean refractions used in astronomy might be correctly computed. But in reality we have no exact knowledge of the variations to which the air is subject in ascending above the surface of the earth. The diffusion of heat and aqueous vapour, the laws which regulate the density and pressure, are but slightly and hypothetically known. Many laborious researches in the lower part of the atmosphere, to which access can be had with instruments, have not been attended with complete success; and they have thrown no light upon what takes place in the upper parts. The limit of the atmosphere, or the height at which the air ceases to have power to refract light, is uncertain, and is, no doubt, as well as the figure of the limiting surface, subject to continual fluctuation. Reflecting on what is said, it must be evident that the mean refraction of a star, which is a fixed quantity, cannot possibly be deduced from an atmosphere daily and hourly varying in its essential properties.

A table of refractions, such as is used in astronomy, contains only mean effects of the atmosphere, that take place at a given point of the earth's surface; and they should properly be compared with other mean effects at the same place. Of these mean effects a principal one is the height that must be ascended in the air for depressing the thermometer one degree, from which another mean effect is easily deduced, namely, the rate at which the density of the air decreases as the height increases. The values of these quantities, as occasionally determined at any particular place, will vary according to the actual state of the air; but a multitude of particular determinations embracing every vicissitude of the atmosphere, will at length lead to mean quantities which are constant, and such as would be observed in the same atmosphere that produces the mean refractions.

It is found that the refractive power of air depends on the density to which it is proportional; and hence the rate at which the density varies at the earth's surface, must have a great influence on the quantity of the astronomical refractions. It furnishes a key to the scale of the real densities in the atmosphere. When a thermometer is elevated in the air, it is found that the mercury continues to be depressed equally to great heights; in like manner the decrements of density will

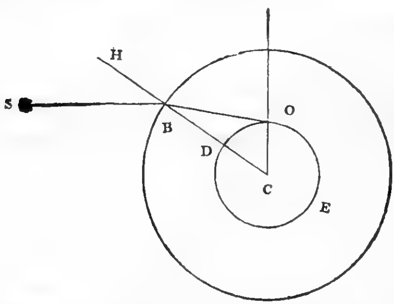
vary slowly from being proportional to the spaces passed through; so that a great share of that part of the astronomical refraction which depends upon the constitution of the atmosphere must be ascribed to the initial rate at which the density decreases. This rate is not hypothetical; it is a real quantity independent of every other; its mean value, which alone we consider, is as determinate and as much the result of experiment as is the refractive power of the air: and in a solution of the problem which is not warped by arbitrary suppositions, and which deduces the effect only from causes really existing in nature, the former quantity will produce a part of the refraction as certain and unalterable, although perhaps not so considerable, as the latter.

But although the initial rate of the decrease of density is an essential element of the astronomical refractions, it may not alone be sufficient for a complete solution of the problem. In ascending to great heights above the earth's surface, the decrements of density will at length cease to be proportional to the spaces passed through, or to the variations of temperature. The refraction of light by the atmosphere is a complicated effect depending upon different considerations: but the influence of these considerations on the mean refractions must be uniform and free from fluctuation, and can arise only from quantities which are constant in their mean values at any proposed observatory. In speaking of mean quantities we exclude whatever is hypothetical, and confine our attention to such only as have a real existence in nature, although it may not in all cases be possible to obtain exact measurements by direct observation. As the refractions themselves are capable of being determined experimentally, they may be made the means of ascertaining what is left unknown in the formula for computing them; and they may thus contribute indirectly to advance our knowledge of the constitution of the atmosphere.

In proceeding to treat of this problem according to the notions that have been briefly explained, it remains to add, that the mean effects of the atmosphere at the same observatory (of which mean effects a table of refractions is one) are alone considered, without at all entering on the question whether such effects are different or not, at different points of the earth's surface. It is very well known that the refractions, to a considerable distance from the zenith, depend only on the refractive power of the air and the spherical figure of the atmosphere; so far there is no reason to doubt that they are the same over a great part of the surface of the globe, according to the opinion generally held by astronomers; but,

at greater zenith distances, when the manner in which the atmosphere is constituted comes into play, it is not so clear that they may not be subject to vary in different climates, and at different localities of the same climate. If a table of refractions at a given observatory contain a set of fixed numbers, these must be deducible from quantities not liable to change, that is, from certain mean effects produced by the atmosphere at the observatory. To trace the relations that necessarily subsist between the mean effects that take place at a given point on the surface of the earth, is the proper business of geometry; if this can be successfully accomplished, the astronomical refractions will be made to depend upon a small number of quantities really existing in nature, and which can be determined, either directly or indirectly, by actual observation.

1. The foundation of the theory of the astronomical refractions was laid by Dominique Cassini. The earth being supposed a perfect sphere, he conceived that it was environed by a spherical stratum of air uniform in its density from the bottom to the top. By these assumptions the computation of the refractions is reduced to a problem of the elementary geometry requiring only that there be known the height of the homogeneous atmosphere, and the refractive power of air. Let the light of a star S fall upon the atmosphere at B, from which point it is refracted to the eye of an observer at O on



the earth's surface D O E: the centre of the earth being at C, draw the radii C O K, C D B H: the angle K O B $= \theta$, is the apparent zenith distance of the star; and O B C $= \phi$ is the angle in which the light of the star is refracted on entering the atmosphere: now from the triangle O B C we deduce

$$\sin OBC = \sin KOB \times \frac{CO}{CB};$$

or, which is the same thing, putting $i = \frac{DB}{CD}$,

$$\sin \phi = \frac{\sin \theta}{1+i}.$$

Again, ϕ being the angle in which the light of the star is refracted, if we put $\delta\theta$ for the refraction, the angle of incidence SBH, which in the present case is always greater than the angle of refraction, will be $= \phi + \delta\theta$; and $\frac{\sin(\phi + \delta\theta)}{\sin \phi}$ will be a constant ratio represented by $\frac{1}{\sqrt{1-2\alpha}}$; so that

$$\sin(\phi+\delta\theta) = \frac{\sin\phi}{\sqrt{1-2\alpha}} = \frac{\sin\theta}{(1+i)\sqrt{1-2\alpha}}.$$

Thus we have the two following equations, which furnish a very easy rule for computing the mean refractions according to Cassini's method, viz.

$$\sin\phi = \frac{\sin.\theta}{1+i}$$

$$\sin(\phi+\delta\theta) = \frac{\sin\theta}{(1+i)\sqrt{1-2\alpha}}.$$

As i and α are both very small numbers, if we put

$$m = i - i^2,$$

$$n = i - \alpha - i^2 + \alpha i - \frac{3\alpha^2}{2},$$

the two last equations will become

$$\sin\phi = \sin\theta - m\sin\theta,$$
$$\sin(\phi + \delta\theta) = \sin\theta - n\sin\theta:$$

and by employing the usual formula for deducing the variation of the arc from the variation of the sine, we get

$$\phi = \theta - m\tan\theta + \frac{m^2}{2}\tan^3\theta,$$

$$\phi + \delta\theta = \theta - n\tan\theta + \frac{n^2}{2}\tan^3\theta:$$

consequently

$$\delta\theta = (m-n)\tan\theta - \frac{m^2-n^2}{2}.\tan^3\theta;$$

that is,

$$\delta\theta = \left(\alpha - i\alpha + \frac{3\alpha^2}{2}\right)\tan\theta - \left(i\alpha - \frac{\alpha^2}{2}\right)\tan^3\theta;$$

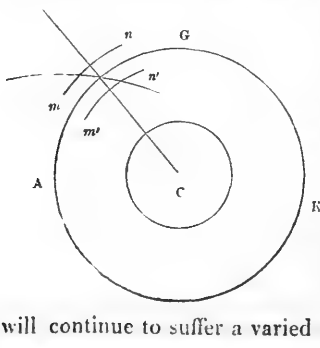
or, which is the same thing,

$$\delta\theta = \alpha\tan\theta\left(1+\alpha - \frac{i-\frac{1}{2}\alpha}{\cos^2\theta}\right) = \alpha(1+\alpha)\tan\theta\left(1 - \frac{i-\frac{1}{2}\alpha}{\cos^2\theta}\right),$$

agreeing exactly with Laplace's formula employed in computing the first part of the table of mean refractions published by the French Board of Longitude.

2. The publication of Newton's Principia enabled geometers to take a more enlarged view of the astronomical refractions, and one approaching nearer to nature. According to Cassini, the atmosphere is a spherical stratum of air, uniform in its density throughout, diffused round the earth to the height of about five miles; in reality the density decreases gradually in ascending, and is hardly so much attenuated as to be ineffective to refract the light at the great elevation of fifty miles. The path described by the light of a star in its passage through the atmosphere is therefore not a straight line, as it would be in the hypothesis of Cassini, but a curve more and more inflected towards the earth's centre by the successive action of air of increasing density. Now in the Principia there is found whatever is necessary for determining the nature of this curve, and consequently for solving the problem of the astronomical refractions, which consists in ascertaining the difference between the direction of the light when it enters the atmosphere, and its ultimate direction when it arrives at the earth's surface. In the last section of the first book of his immortal work, Newton teaches in what manner the molecules of bodies act upon the rays of light and refract them; and as the atmosphere must be uniform in its condition at all equal altitudes, its action upon light can only be a force directed to the centre of the earth; so that the trajectory in which the light moves, being described by a centripetal force, the determination of its figure will fall under the propositions contained in the second section of the same book.

Conceive that light falls upon an atmosphere A G K, constituted as Cassini supposed, spherical in its form, concentric



to the earth, of the same density ϱ throughout; and suppose that the attractive force of the molecules of air situated in the surface A G K extends to $m\,n$ on one side, and to $m'\,n'$ on the other. Every molecule of light when it arrives at $m\,n$ will be attracted by the air in a direction perpendicular to the surface A G K, and tending to C the centre of the earth; it will continue to suffer a varied attraction till it penetrates to

the other surface $m' n'$; but when it has passed this limit, it will no longer be acted upon effectively by the surrounding air, which will attract it equally in all opposite directions. As the attraction of air extends only to insensible distances, in estimating its action upon a molecule of light we may consider the limiting surfaces $m n$ and $m' n'$ as parallel planes, the forces being perpendicular to $m n$, and of the same intensity at all equal distances from it. The law of the forces in action between $m n$ and $m' n'$ is indetermined; it may be uniform, or varied in any manner. These things being premised, it follows from a fundamental proposition of the philosophy of Newton, the demonstration of which it would be useless to repeat here, that the total action of all the forces between $m n$ and $m' n'$ is to add to the square of the velocity of the light incident at $m n$, an increment which is always the same, whatever be the direction in which the light arrives at $m n$. If we now put v for the velocity with which the light enters $m n$, and v' for the velocity with which it leaves $m' n'$, what is said will be expressed by this equation,

$$v'^2 - v^2 = 2 \,.\, \phi\,(\varrho),$$

$\phi\,(\varrho)$ denoting the sum of all the forces between $m n$ and $m' n'$, each multiplied by the space through which it acts, a sum which, in different atmospheres, will vary only when ρ varies.

It will be convenient to have a name for the function $\phi\,(\rho)$, and the most appropriate term seems to be, the refractive power of the air. In using this term, or in expressing by $\phi\,(\rho)$ the action of air upon light, it is always supposed that the light passes out of a vacuum into air of the density ϱ.

A property resulting from what is said may be mentioned. Having drawn a radius from the centre of the earth to the point at which the light falls upon the atmosphere, let ϖ denote the angle made by the direction of the velocity v with the radius, and ϖ' the angle made by the direction of the velocity v' with it; then $v \sin\varpi$ and $v' \sin\varpi'$ will be the partial velocities of the light parallel to the surface of the atmosphere. Now these are equal; for all the forces which change v into v' are perpendicular to the surface of the atmosphere, and therefore they have no effect to alter the velocity of the light parallel to that surface. Thus

$$v \sin\varpi = v' \sin\varpi',$$

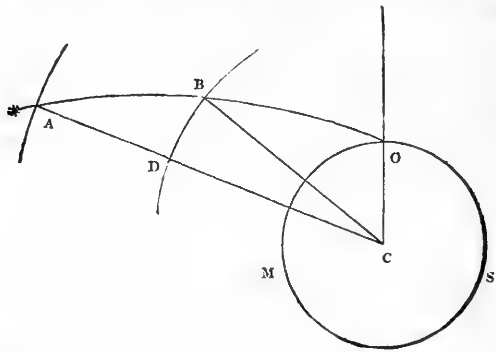
and

$$\frac{\sin\varpi}{\sin\varpi'} = \frac{v'}{v},$$

that is, in words, the ratio of the sine of incidence to the sine of refraction is equal to the ratio of the velocity of the

light after refraction to the velocity of the incident light; which ratio, being independent of the direction of the incident light, is constant for all light that falls upon the atmosphere with the same velocity.

What has been said of an atmosphere supposed homogeneous is next to be applied to the real atmosphere of the earth, the density of which decreases continually in ascending. The sphere M O N of which C is the centre, representing the



earth, let S A B O be the trajectory described by light emanating from the star S, in its passage through the atmosphere to the earth's surface at O; through any two points of this curve, A and B, draw spherical surfaces concentric to the earth, the distances A C and D C from the common centre being $r + dr$ and r. Representing by ρ the density of the air above the spherical surface at A, let $\varrho + d\rho$ stand for the density, supposed uniform, of the stratum between the two surfaces at A and B: and it is to be observed that, though $AD = dr$ is an infinitesimal, it is nevertheless to be accounted infinitely great when compared to the insensible distance at which the molecular action of the air at A ceases to act: from which it follows that the refractive power of the stratum upon light which enters at A, is exactly equal to the refractive power of a homogeneous atmosphere, supposing the density $\rho + d\rho$ to extend unvaried to the earth's surface. Now if v denote the velocity with which the light moves in the trajectory at A, the refractive power of the air above the stratum will diminish v^2 by the quantity $2\,\phi\,(\rho)$; for it is obvious that the refractive power of the air above the spherical surface at A, is equal and opposite to the refractive power of a homogeneous atmosphere within the same surface and of the

density ρ: on the other hand, the refractive power of the stratum will augment v^2 by the quantity $2\,\phi\,(\rho + d\,\rho)$: wherefore, upon the whole, the real increment of v^2 will be $2\,\phi\,(\rho + d\,\rho) - 2\,\phi\,(\rho)$; so that we shall have

$$d\,.\,v^2 = 2\,\phi\,(\rho + d\,\rho) - 2\,\phi\,(\rho) = 2\,d\,.\,\phi\,(\rho);$$

and, by integrating,

$$v^2 = n^2 + 2\,\phi\,(\rho).$$

It is obvious that n^2 is the square of the velocity of the light before it arrives at the atmosphere, that is, when it moves in a vacuum. We may consider n^2 as the unit in parts of which the squares of the velocity of the light at the several points of the trajectory are estimated; which requires that the formula be thus written,

$$v^2 = 1 + 2\,\phi\,(\rho).$$

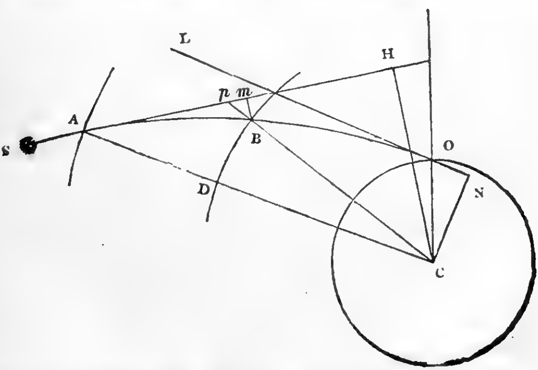
Resuming the equation

$$d\,.\,v^2 = 2\,d\,.\,\phi\,(\rho),$$

we have

$$v\,d\,v = d\,.\,\phi\,(\rho) = \frac{d\,.\,\phi\,(\rho)}{d\,r}\,.\,d\,r;$$

from which we learn, that the same addition which v^2 receives by the refractive power of the air at A, it will acquire by the accumulated action of the force $\frac{d\,.\,\phi\,(\rho)}{d\,r}$ at all the points of the line $d\,r$, or, which is the same thing, by the action of the force $\frac{d\,.\,\phi\,(\rho)}{d\,r}$ urging the light towards the earth's centre at all the points of the curve A B. Thus the path of the light of a



star in its passage through the atmosphere is a trajectory de-

scribed by the action of the centripetal force $-\frac{d \,.\, \phi(\rho)}{d r}$ tending to the centre of the earth, the sign $-$ being necessary, because the analytical expression is essentially negative.

Draw A H a tangent of the curve at A, B m perpendicular to A H, and produce C B to meet A H in p: put n for the angle A C O which the radius vector A C makes with C O the vertical of the observer; $d z$ for A B the element of the curve: $d \tau$ for the time of moving through A B; and R for the radius of curvature at A. Now Bp is the space through which the centripetal force $-\frac{d \,.\, \phi(\rho)}{d r}$ would cause a molecule of light to move from a state of rest in the time $d \tau$: wherefore

$$2 \mathrm{B}p = -\frac{d \,.\, \phi(\rho)}{d r} \,.\, d \tau^2;$$

also

$$\mathrm{B}p = \frac{\mathrm{B}m}{\sin \mathrm{BAD}} = \frac{d z^2}{2 \mathrm{R}} \,.\, \frac{d z}{r d n};$$

and, by equating the equal quantities, we get

$$\frac{d z}{\mathrm{R}} = -\frac{d \,.\, \phi(\rho)}{d r} \,.\, \frac{d \tau^2}{d z^2} \,.\, r d n \quad \ldots\ldots\ldots\ldots \quad (1.)$$

The refraction of the light in moving from A to B, or the difference of the directions of the curve at A and B, is evidently equal to the angle subtended by A B at the centre of the circle of curvature, that is, to $\frac{d z}{\mathrm{R}}$: wherefore if $\delta \theta$ represent the refraction increasing from the top of the atmosphere to the earth's surface, we shall have

$$d \,.\, \delta \theta = -\frac{d \,.\, \phi(\rho)}{d r} \,.\, \frac{d \tau^2}{d z^2} \,.\, r d n.$$

This formula is merely an application of the 6th proposition of the first book of the Principia.

Another general and useful expression of the differential of the refraction is easily obtained. Draw C H $= y$, perpendicular to the tangent A H: from the known properties of curve-lines, we have

$$\mathrm{R} = \frac{r d r}{d y}:$$

wherefore

$$\frac{d z}{\mathrm{R}} = d y \,.\, \frac{d z}{d r} \,.\, \frac{1}{r} = \frac{d y}{\sqrt{r^2 - y^2}}:$$

consequently

$$d\,.\,\delta\,\theta = \frac{d\,y}{\sqrt{r^2 - y^2}} \quad \ldots\ldots\ldots\ldots\ldots\ldots \quad (2.)$$

but in this formula $\delta\,\theta$ must be conceived to increase from the surface of the earth to the top of the atmosphere.

In applying the last formula it is necessary to have a value of y. Draw O L to touch the curve at O, and C N perpendicular to O L: put $\upsilon\,\rho'\,'$ for the density of the air, and the velocity of the light at O; also y' for the perpendicular C N, a for C O the radius of the earth, and θ for angle C O N, which is the apparent zenith-distance of the star: we shall have

$$\frac{\text{Area A B C}}{d\,\tau} = \frac{d\,z}{d\,\tau} \times y = \upsilon \times y:$$

and because the curve is described by a centripetal force tending to C, the value of $\upsilon \times y$ will be the same at all the points of the curve; wherefore

$$\upsilon \times y = \upsilon' \times y':$$

and

$$y = y' \times \frac{\upsilon'}{\upsilon} = a \sin\theta \times \frac{\upsilon'}{\upsilon}\,.$$

Now, according to what was before shown,

$$\upsilon = \sqrt{1 + 2\,\phi\,(\rho)},$$
$$\upsilon' = \sqrt{1 + 2\,\phi\,(\rho')};$$

wherefore

$$y = a \sin\theta \times \sqrt{\frac{1 + 2\,\phi\,(\rho')}{1 + 2\,\phi\,(\rho)}}\,. \quad \ldots\ldots \quad (3.)$$

By substituting this expression in the differential of the refraction, the problem will be reduced to an integration.

The equations that have been investigated are perfectly general, and will apply in any constitution of the atmosphere that may be adopted. It has been thought better to consider the manner in which the forces act, than to employ functions with peculiar properties to express the molecular action. When the light in passing through the atmosphere arrives at a surface of increased density, it receives an impulse which may be considered as instantaneous; and this impulse being distributed over the breadth of a stratum of uniform density, ascertains the centripetal force tending to the earth's centre, by the action of which the trajectory is described.

[To be continued]

XLV. *Memoir of* G. Moll, LL.D., *Professor of Physical Sciences in the University of Utrecht, and Member of the Academy of Brussels.* *By* M. Quetelet.*

THE sciences, like those who cultivate them, enjoy this happy privilege; that in regard to them there exist no political barriers, no national antipathies, nor even those kinds of intellectual frontiers which, under the influence of languages, become established between the literatures of different nations. They form a true republic, whose peace nothing should ever disturb, and where merit alone leads to distinctions, for the conferring of which there are needed no formal resolves nor protecting regulations. The respectgiven to talents, like the esteem accorded to virtue, is so inherent in our nature, that it is not in our power to refuse it when it is really deserved. As for us, members of this Academy, who, by our studies and our tastes, find ourselves placed in peaceful regions, where the shocks of political dissensions cease to be felt, we shall see in the philosopher who is the subject of this memoir only the companion who took his place in the midst of us, who took part in our labours and aided us by his knowledge.

Gerard Moll was born at Amsterdam on the 18th of January 1785. His parents, Gerard Moll and Anna Diersen, whose only child he was, concentrated all their affections in him. His father was engaged in commerce and was more than usually well-informed: his mother especially possessed a very cultivated understanding and a taste for poetry, to which she applied herself with success; it was she who attended to the education of her son, and who watched and aided every dawning of his intelligence. When he entered the schools his progress was very rapid, and he was scarcely ten years old before he spoke with facility, not only his mother tongue, but the French and German languages. He had not neglected the rudiments of Latin; but as he was intended to a life of trade and business, the language of Cicero had to give place to that of Watt.

The youthful Gerard Moll was placed as an apprentice in

* From the *Annuaire de Bruxelles* for 1839. The following note is subjoined.

In writing this memoir we have principally availed ourselves of the particulars derived from an obituary article published by Professor Van Rees, (see Nos. 35 and 36 of the *Letterbode* for 1838) and in the tract, *L. G. Visscher Oratio de Gerardo Moll*, read before the University of Utrecht, the 26th of March 1838. M. Van Rees is one of M. Moll's most distinguished pupils, and more than once rendered valuable assistance to his master, whose worthy rival he had become. No one certainly was more worthy than M. Van Rees of succeeding him in the duties of professor and director of the Observatory of Utrecht.

one of the commercial houses in his native city. His frequent visits on ship-board, his intercourse with sailors, and his natural curiosity, soon made him familiar with numerous particulars connected with the art of navigation, and at the same time created in him a taste for the mathematical sciences.

To the care of Professor Keyser he owed his initiation in the secrets of geometry and algebra: in 1801 he began the study of astronomy, which notwithstanding its utility was at that time rather neglected in his country; but it was not till 1804 that his taste for this science decidedly showed itself. He was then in a commercial house in London, but his inclination led him far less to the counting-houses of merchants than to the workshop of the celebrated instrument-maker Troughton, whose acquaintance he had succeeded in obtaining. After a time he had procured a sextant of ten-inch radius; and rich in this treasure, he thought henceforth he had done enough for trade, and resolved to abandon its pursuits.

He now went back to Holland; it was at the time when the conscriptions decimated the populations of the interior, in order to supply abroad the armies of the empire, which fought in different parts of Europe with incredible intrepidity and activity, and loaded themselves at once with glory, with booty, and with the curses of conquered nations. The father of our young philosopher, who felt no ambition that his son should take part in these conquests, sought for the means of keeping him at his side, and by destining him for scientific pursuits thought to find the expedient he wished for. He caused him therefore to be entered as a student at the Athenæum of Amsterdam, where the young Gerard eagerly attended the lectures of Cras and Van Lennep for literature, and those of the celebrated Van Swinden for the sciences. His connexion with this latter philosopher completely decided his destination. Our colleague had also made acquaintance with Professor Van Beeck Calkoen; and at his desire he took part with Professor Keyser in determining the difference of the meridians of Amsterdam and Utrecht by means of fire signals made on the top of the tower of Loene*.

It was in 1809 that he took the degree of Candidate in Philosophy at the University of Leyden. The following year, in the month of June, he went to Paris, in order to pursue his favourite studies with more activity, and upon a greater theatre. He became acquainted with several distinguished philosophers, and particularly with Delambre, for whom he always professed sentiments of lively gratitude and of sincere

* *Letterbode*, 1807, i. 21.

attachment. The declining health of his father, who died soon after, recalled him to his country in the month of February 1812.

Prof. Van Beeck Calkoen had also deceased at Utrecht, and the chair of mathematical and physical science had become vacant. A fear was entertained that it would be suppressed, through the measures taken by the French government, which had lowered the University of Utrecht to the rank of a *secondary school*. The influence of Delambre and Van Swinden, however, were powerful enough to obtain the nomination of our colleague, first as director of the Observatory of Utrecht, and some months after as professor of mathematical and physical sciences. At the reorganization of the University, in 1815, he obtained the chair of physical sciences that Professor Rossyn had filled.

The first care of Moll in his new office was given to putting in order the observatory, which was in a bad state in consequence of the past misfortunes. The ruins of an old isolated tower situated on one of the ramparts of the town had been made use of for the construction of this scientific establishment. The wisest plan no doubt would have been to demolish this elevated building, which had neither the requisite solidity, nor suitable conveniences; but money was wanting, and the success of this observatory was completely compromised. In vain did the new director fix in succession a choice of the finest instruments which he had been able to procure in the different visits he made to England and Germany; all his efforts failed before difficulties which taken separately would have been of little importance, but the combination of which could not but, in the long run, operate most unfavourably. The first of these disadvantages, and perhaps the most serious, was the distance from home that the astronomer had to go to the place for his observations. A small and inconvenient spot, elevated and by no means firm bases, which would only admit of instruments of inferior dimensions, and of taking in hand a restricted number of observations, presented obstacles which ended without doubt in quite subduing the zeal which our colleague had shown at the beginning of his enterprise. The number of his astronomical labours were indeed few; he published only his observations of the comet of 1819 *, and those of the transit of Mercury over the sun in the month of May 1832†;

* *Letterbode*, 1819, i. 59.

† T. iv. 71. of the Memoirs of the 1st class of the Institute of the Low Countries.

he also communicated to the Royal Astronomical Society of London, the whole of the observations that were made in Holland of the fine eclipse of the sun in September 1820, at which time M. Moll was himself in England*. The preceding year, when he resigned the rectorate of the University of Utrecht, he had published a Latin dissertation on the ulterior progress of astronomy, which is inserted in the annals of this learned body.

That which, moreover, seems to have contributed most to turn aside our colleague from his astronomical labours was the versatility of his mind, which loved to spread itself at once over a great number of objects; it was this desire of change of place and of keeping pace with the progress of science, which led him to visit his neighbours, and especially the English, as often as his duties of professor allowed him, and we should add, almost always longer than these duties permitted. The government, however, well understood that the inconveniences which resulted from this were very trifling in comparison with the advantages to be derived from these repeated excursions, and had the wisdom to wink at what might be irregular in his conduct as professor. As a natural philosopher Moll was truly at home; there are labours of his which can leave no doubt in this respect; but the service in which he has been most especially useful to his country was in keeping her acquainted with all that was doing abroad, not only in the sciences, but in all the uses to which they may be applied for the wants of society. Was any important discovery made, any useful improvement, he not only hastened to communicate it in lectures and in the scientific societies to which he belonged, but he endeavoured through the journals to make the results comprehensible to the generality of readers; thus, navigation by steam-boats, artesian wells, warming hot-houses by steam, submarine discoveries by the diving-bell, the construction of lightning conductors, each in its turn found in him a zealous patron always disposed to make their advantages available †. He loved especially to throw light on the scientific subjects, so to speak, in the order of the day, and to which circumstances gave an interest; thus, immediately after the burning of a part of the beautiful church of St. Bavon at Ghent, he presented his remarks on the improvement of fire-engines. On the subject of horse-races, he communicated in a notice a statement of the swiftness of horses of different countries and of different breeds. If any remark-

* Vol. i. 144, Memoirs of the Royal Astronomical Society.

† Memoirs on these different subjects have been inserted by M. Moll in the *Letterbode* and in the *Mémoires de la Société de Haarlem.*

able change in the atmosphere occurred, he took the opportunity of communicating his observations on this subject, and of calling to mind the analogous phænomena which had formerly taken place. These services in detail greatly contributed to make his name popular with his countrymen.

M. Moll was the soul of all the scientific commissions that the government formed, and it must be allowed that he was able to render more useful services than in his little observatory, where indeed for a long time the only help he had was his porter.

Immediately after the formation of the kingdom of the Netherlands, the system of weights and measures had to be settled, and M. Moll was one of the principal members of the commission employed in this work. In this instance the government bestowed on him a mark of its satisfaction by giving him the title of knight of the order of the Belgic Lion.

He had above all an opportunity of affording proofs of his practical knowledge in the commission which was employed to make a report on the state of the waters, and of their drainage, in the northern provinces. The work required was difficult and of the highest importance. Every one knew that the bed of the rivers was imperceptibly raised in Holland, and that their mouths were filling more and more with sand; but opinions were singularly divided upon the means of remedying an evil which tended some time or other to swallow up a considerable portion of the country. Some advised giving more elevation and weight to the dykes; others advised lowering them; others in fine were of opinion that channels of drainage should be made laterally; but they differed among one another on the means of executing this no less than the former. The commission was therefore employed to examine all these projects, and to propose such plans for the security of the country as should unite financial interests with those of commerce and industry. This labour of our colleague lasted four years, and he gave his time almost exclusively to it. He was appointed the reporter of the commission, and all are agreed in regarding his work, which was printed in 1827, as a model of order, clearness, and judgement.

In 1826 he had been nominated on another commission for the amelioration of marine charts, and for the examination of officers. He was no less useful in these new duties; for, as we before noticed, from his childhood he had been naturally led to the study of navigation and all that is connected with it. This branch of knowledge was the more congenial to his taste because it was intimately connected with

the causes of the prosperity and the glory of Holland; and M. Moll was no less a good patriot than an enlightened philosopher. His work on the early maritime expeditions of the Netherlands, *Vroegere Zeetogten der Nederlanders*, is a national work full of curious and useful inquiries, and breathing the purest love of country. Whilst setting forth the immense services rendered to navigation by Dutch voyagers, the author is far from contenting himself with emphatic praises; on the contrary, he reproaches his fellow-citizens for having fallen off from the condition to which their ancestors had raised them, and encourages them to strive to regain their ancient splendour.

We owe to him also some interesting notes with which he enriched the work of M. Van Kampen* on the history of the sciences in the Netherlands†. His scientific acquirements and his taste for literature naturally led him into the domain of history. Influenced at the same time by sentiments of gratitude, he wrote in succession biographical notices on Delambre, Keyser, and Van Swinden, who had been his masters; he also paid a tribute of esteem to the memory of Delaplace and Wollaston, with whom he had been intimate. During the latter part of his life he was occupied about a memoir of our old colleague and his countryman the baron Van Utenhoven, and which must have been printed subsequently.

When the government, in 1835, resolved, at the request of the English government, and in aid of the labours of Messrs. Whewell and Lubbock, to cause a series of observations to be made upon the hours and height of the tides along the coasts of Holland, it was to M. Moll also that the care of directing and superintending them was entrusted. In volume vii. of the Memoirs of the 1st class of the Institute may be seen the report which was made on this subject. This undertaking could not have been placed in better hands than those of the philosopher, who, at a former period, had furnished the most judicious remarks upon the placing of the standard scale employed to mark the height of the level of the sea before Amsterdam (*het Amsterdamsche peil*).

It remains for me now to speak of the labours of M. Moll in the physical sciences. The situation of this philosopher afforded him considerable advantages, of which he knew how to avail himself with skill. Beside the collections of the ob-

[* We just learn, with sincere regret, that science has sustained another severe loss in the death of this distinguished professor, in the prime of life, the very day before Oxford sustained a similar loss in the death of the honourable and excellent Rigaud.—Edit.]

† *Bijdragen tot de geschiedenis der wetenschappen in Nederland.*

servatory and those of the cabinet of natural philosophy of the university, which by his care had increased considerably, he had also at his disposal the collections of the society of sciences of Utrecht, which were not less rich. A circumstance very honourable for our colleague enabled him to add still more to the treasures that he had at command: by the death of Professor Ekama, in 1826, the chair of physical sciences in the university of Leyden had become vacant, and the curators of this establishment had made an overture to Moll to induce him to fill it. These honourable offers had not been positively rejected; but the university of Utrecht felt that their honour and interest would be compromised by permitting a philosopher who shed so much lustre upon it to remove. M. Moll yielded to the solicitations addressed to him by the university, and resolved to remain in his situation. The city of Utrecht wished to present him with a testimony of its gratitude; but our colleague refused to accept anything for himself, he only expressed a desire to see something done for the interests of science, and the government of the city placed at his disposal the sum of ten thousand florins for the purchase of instruments.

One of the most important labours of M. Moll was that which he accomplished in conjunction with M. Van Beek upon the velocity of sound. The experiments of these philosophers took place in 1823, a year after those which were made by a commission of the Bureau of Longitude of France, composed of MM. Arago, Gay-Lussac, De Humboldt, &c. The government placed at their disposal all the necessary means of execution; and the base that the sound had to traverse extended over a length of 17,000 metres, between Kooltjesberg near Naarden and the elevation named the Seven Trees (*zeven boomen*), near Amersfort. Six nights were devoted to these experiments, which were made with a care which seemed to leave nothing to be desired. The results of them were recorded in the Memoirs of the Institute of the Low Countries *. The Royal Society of London, by inserting them in its Transactions, also proved the interest it attached to them.

M. Delaplace had also advised them to communicate their labours to the Bureau of Longitude; and I myself was entrusted with a letter, in which that illustrious philosopher obligingly invited the professor of Utrecht to make this communication.

The inquiries of Œrsted, in 1819, relative to the action which

* *Mémoires de l'Institut des Pays Bas,* vii. 281; and Philosophical Transactions, I. 823. 2nd part.

an electric current exercises on the magnetic needle, opened a new field, upon which the philosophers of all nations eagerly entered, and which soon, thanks to their united efforts, was as much cultivated and as productive as any other part of the vast domain of natural philosophy. Our colleague was not one of the last to put his hand to the work, and he communicated in the *Journal de Physique* * the different results at which he had arrived by repeating and extending the experiments of the Danish physicist. He endeavoured to show that there is a difference of action in chemical and magnetic phænomena, according as the electricity is developed by simple contact, or by the apparatus of Wollaston. He was occupied for several years in these researches.

The English experimentalist Sturgeon had made known, in 1826, that a bar of soft iron bent into the form of a horse-shoe and covered with a spiral wire of copper, becomes a powerful magnet as soon as the extremities of the wire are placed in contact with the poles of a galvanic pile, and that it instantaneously loses its power as soon as the contact ceases. In an experiment made in England, and at which M. Moll was present, one of these temporary magnets bore nine pound weight. Our colleague resolved to make the experiment on a larger scale. For this purpose he used a plate of zinc of a surface of eleven square feet, dipping into a narrow copper vessel, and he put the poles of this element of a galvanic pile in connexion with the ends of a copper wire rolled 83 times around a soft iron bent into the form of a horse-shoe and weighing five pound. As soon as the contact was established the iron was able to bear 50 pound, and it was even possible to carry the charge to 76 pound.

Not content with these first results, M. Moll had an iron constructed of 29 pound weight; and with the same galvanic element which he had used at first he made it bear 295.

I repeated these different experiments with M. Lipkens, inspector-general of ordnance; the results to which we came have been recorded, together with an extract from M. Moll's work, in volume vi. of the *Correspondance Mathématique*, p. 327. We were at the same time led to investigate the most advantageous proportions which it is proper to give to the horse-shoe and to the voltaic element in order to produce the *maximum* of effect†. Expressing his desire to see

* By M. De Blainville, v. xcii. p. 295, 309 and 311.

† *Correspondance Math.*, vol. vii. p. 54, and following. This is perhaps the place to complain of an error in an Italian Journal, made to the prejudice of our colleague. In page 63 and following of vol. iii. of the *Annali delle Scienze* of Padua, we read an article directed against the

fresh inquiries made upon this interesting subject, Moll replied to this appeal, by quoting some of his experiments which we had not had it in our power to become acquainted with on account of the state of war which existed between the two countries, and by producing new results obtained by means of very small elements*.

We owe to M. Moll several other labours in the department of physical science, and amongst others a memoir on reflecting telescopes †, a subject which engaged much attention at that time in Holland, as we have mentioned in our memoir on the Baron Van Utenhoven ‡; researches on the degree of temperature at which water reaches its *maximum* of density §; comparative observations between the kilogram and Dutch, English, and other weights ||.

At the time when there appeared in England a work ¶ which made a great noise because several of the first scientific men were attacked in it without reserve, Moll undertook the defence of the injured party, and declared himself its champion. His publication on this subject was entitled "*On the alleged Decline of Science in England, by a Foreigner* **." If we consider the eminent services rendered to science in general, and to the applied sciences in particular, by the English nation,

Annales de Physique of MM. Arago and Gay-Lussac, and against the *Correspondance*, for not having mentioned, relative to the subject of M. Moll's labours, the inquiries of Professor Dal Negro, which, it is said, were presented to the Academy of Padua *the 21st of June and the 10th of July of the year* 1831. Now, a single word must put an end to these charges, founded, as we would fain believe, upon an accidental error of date. The experiments of Moll were communicated to the Institute of Amsterdam *the 30th of January* 1830, and published immediately after by this learned body under the title of *Electro magnetische proeven*, in 8vo, by Muller, Amsterdam, 1830. The results of them were recorded not only in the *Correspondance Mathém.* of 1830, and in the *Annales* of MM. Arago and Gay-Lussac, but also in the *Bibliothèque Universelle*, v. xxxxv. p. 19; in the Edinb. Journal of Science, No. vi. v. iii. p. 289; in the Journal of the Royal Institution, 1831, p. 379, &c. Should we not be right in replying to the Italian author in his own words, leaving him to be responsible for whatever they contain that is bitter and disdainful? *Prima di fare altri experimenti, dovrà studiare le cose già pubblicate in Olanda, in Francia, in Inghilterra, in Genova, etc.*

* *Brief betrekkelijk eene aanmerking van den heer Quetelet, Letterbode*, 1833, i. 82. *et Bibl. Univ.* June 1833.

† *Mém. de l'Institut des Pays-Bas*, 1st class, i. 29.

‡ *Annuaire de l'Académie de Bruxelles* for 1838. p. 64.

§ *Bijdragen tot de Nat. Wetens.*, i. 241. || *Ibid.*, vi. 119.

¶ On the Decline of Science in England. By Ch. Babbage, 8vo, 1830.

** London, 1831. [As our national honour and gratitude are concerned in the diffusion and preservation of this able and interesting Tract, which had but a very limited circulation, we take this opportunity to apprise our readers that copies are to be had of Mr. Wacey, successor to Boosey, Broad-street, and at the office of the Philosophical Magazine.—EDIT.]

we cannot but approve the generous warmth which induced our colleague to compose his work. With regard to ourselves, we are certainly far from adopting the judgements of the English author on some of his countrymen; and the sincere friendship which we feel for him has made us regret to find often too much asperity where matters of science were the only subjects of discussion; at bottom, however, we can see in it only one of those freaks which men of superior talent sometimes allow themselves to play, and the particular object of which is to stimulate the ardour of a nation. These are some of those family reproaches which strangers should not take for serious.

M. Moll however had much to congratulate himself upon in his relations with the English philosophers, who had given him multiplied proofs of their esteem. He belonged to many of their learned societies, and in 1835, at the meeting of the British Association at Edinburgh, he was made a member of it, at the same time that the university offered him the degree of doctor of laws, *honoris causa*, and that the freedom of the city was conferred on him; the following year the meeting took place in Dublin, and the university of that city also presented him with a diploma of doctor of laws.

The upright and firm character of M. Moll, his generally polite manners, and his obliging disposition, had obtained for him numerous friends both in his own and in other countries; his tastes drew him towards the English, for whom he strongly expressed his lively sympathy. He may perhaps be blamed, on the other hand, as yielding sometimes, when individuals were in question, to prepossessions, which did not influence him in matters of science; and these prepossessions were the more apparent as they were evinced in a somewhat rough and pungent manner*.

Moll was named member of our Academy the 7th of May, 1828: he had given us reason to hope from him an active cooperation in our labours, but the political events which ensued gave him but small opportunity of realizing his promises.

The 11th of December 1837, our colleagne celebrated the twenty-fifth anniversary of his professorship; his colleagues, his pupils, and his numerous friends gave him touching proofs of their attachment and esteem upon this occasion. But this day, devoted to pleasure, which consecrated so remarkable an

* Mollium in dijudicandis aliis semper æquitate ductum fuisse, nolo equidem effari. Hoc quidem dicere oportet, æquum judicem eum extitisse si non cunctis, certe multis, optimis, in primis vero senibus et præceptoribus suis. *Oratio de G. Moll.*

epoch of his academic career, was destined also to mark its termination. M. Moll died thirty-seven days afterwards, on the 17th of January 1838, in his native city, and at the house of his friend M. F. A. Van Hall, where he had gone to spend his winter holidays. His remains, according to his desire, were carried to Amerongen, where also repose those of his mother. According to his will, his instruments, and his library, which was very rich, were bequeathed to that university, of which he had been one of the chief supports.

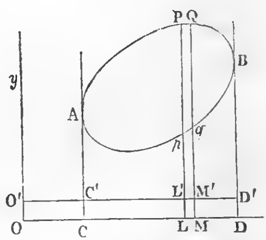
A. Quetelet.

XLVI. *A Note on Definite Double Integration, supplementary to a former paper on the Motion and Rest of Fluids.* By J. J. Sylvester, *Professor of Natural Philosophy in University College, London.**

In a paper on Fluids which appeared in the December Number of this Magazine, I had occasion to remark, that the mass of an area having at the point (x, y) a density $\frac{du}{dx} + \frac{dr}{dy}$ could be expressed by the simple formula

$\int_l^0 \left\{ u \frac{dy}{ds} - v \frac{dx}{ds} \right\} ds$; l being the length, and ds an element of the bounding curve: this may be thought to require some explanation.

Fig. 1.



1. Let A P B q represent any oval.

P p L, A q M any two contiguous ordinates cutting the curve in P p Q q respectively, A C, B D the two extreme tangents parallel to O q, and ϱ the density at any point (x, y). The expression $\int\int \varrho \, dx \, dy$ will serve to denote the mass of the oval area A p B q, and the limits may be twice taken, i. e. 1. the two values of y corresponding to any one of x; and, 2. the two values of x corresponding to C and D. This

* Communicated by the Author.

method is in fact tantamount to taking the sum of the columns P p, q Q; but this is not necessary, for A p B q may be considered as the *algebraical* sum of the mixtilinear area A P Q B D C, and the mixtilinear area B D C A $p\,q$, or (if any line O′ C D′ be drawn parallel to O C L M D) of A P Q B D′ C and B D′ C′ A $p\,q$.

Thus then the mass $= \int d\,x\,(\int \varrho\,d\,y)$, $\int \varrho\,d\,y$ being left indeterminate, and the extremity of x travelled round from C to D, and back again from D to C.

This will be better expressed by transforming the variable, and summing with respect to some quantity, such as the arc of the curve, which continuously increases, or if we please, with respect to θ, the angle subtending any point taken within the curve.

The mass is then $= \pm \int_{2\pi}^{0} d\,\theta \left\{ (\int \varrho\,dy) \cdot \frac{d\,x}{d\,\theta} \right\}$; always

remembering that *no* constant need be added to $\int \varrho\,d\,y$, and that the doubtful sign arises from the choice of ways in which θ may be measured round. If the area be not included by one line; but by several, as for example, by a curve and a right line, the above integral, if broken up into as many parts as there are breaches of continuity, will still apply.

2. Let us suppose that we have two areas exactly coinciding with, and overlapping one another; but the density of the one at (x, y) to be ϱ, and of the other ϱ'.

Let the mass of the first be treated as the sum of columns parallel to O y, and that of the second as the sum of columns parallel to O x.

The one will be represented by $\pm \int_{2\pi}^{0} d\,\theta\ (\int \rho\,dy)\,\frac{d\,x}{d\,\theta}$

the other will be represented by $\pm \int_{2\pi}^{0} d\,\theta \int (\rho'\,d\,x)\,\frac{d\,y}{d\,\theta}$

and the sum of the two, or the joint mass, by

$$\pm \int_{2\pi}^{0} d\,\theta \{\int \rho\,dy\}\,\frac{d\,x}{d\,\theta} \pm \int_{2\pi}^{0} d\,\theta\,\{\int \rho'\,d\,x\}\,\frac{d\,y}{d\,\theta}$$

So long as these two operations are performed separately, the doubtful signs may be preserved in each term, because s need not be travelled round in the same direction for the two summations; but if we perform the second integration conjointly for the two masses, their sum

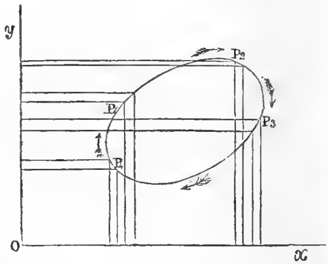
$$= \pm \int_{2\pi}^{0} d\,\theta \left\{ \{\int \rho\,d\,y\}\,\frac{d\,x}{d\,\theta} \pm ?\ (\int \rho'\,d\,x)\,\frac{d\,x}{d\,\theta} \right\}$$

the mark of interrogation denoting that *one or the other*, but

not *either* of the signs $\pm$ must be used, and the question is, which?

This will be answered by taking different points in the bounding line, which may be continuous or not. Now every line returning into itself, whether continuous or not, will naturally divide with respect of any given system of axes, into at most four parts, or sets of parts; two in which dx and dy both increase or both decrease, and two in which one increases and the other decreases.

Take P_1, P_2, P_3, P_4, any points in the four quadrants respectively, it will be observed that,



At P_1 the ρ column enters additively, and the ρ' column subtractively.

At P_2 both columns are additive.

At P_3 the ρ' column is additive and the ρ column subtractive.

At P_4 both columns enter subtractively.

Again, reckoning round in the direction of the arrows,

At P_1, x and y are both increasing.

At P_2, x is increasing and y decreasing.

At P_3, x and y both decrease.

At P_4, x is decreasing and y increasing.

Thus when $\int(\rho\, dy)$ and $\int(\rho'\, dx)$ are affected with the same signs, dx and dy are of opposite signs; and when $\int\rho\, dy$, $\int\rho'\, dx$ are of opposite signs, dx and dy are of the same sign.

Hence it appears that the mass of the area, whose density at (x, y) is $\rho + \rho'$, V is capable of being represented by

$$\pm\int_{2\pi}^{0} d\theta \left\{(\int\rho dy)\frac{dx}{d\theta} - (\int\rho' dx)\frac{dy}{d\theta}\right\}.$$

XLVII. *Notice of a Mineral Spring, Menero Downs, N. S. Wales. By Dr.* F. Lhotsky, *late of the C. S. Van Diemen's Land.**

IN the small tract of country as yet known of New Holland, a number of interesting facts relating to mineralogy and

* Communicated by the Author.

geology have been discovered, although geology being but a new science in Europe, has but very lately reached that newest continent. The burning mountain described by Mr. Wilson, the remains of a crater seen by Major Mitchell, and the mineral spring I am about to notice are among the most interesting data in this department.

The spring is situated 300 miles from Sydney, amongst extensive uninhabited downs, which skirt the Australian Alps on their eastern side. The formation about the spring is calcareous, and the immediate neighbourhood of it is formed by an extensive level of travertine, just like that with which the plains of Romagna are covered. The spring forms (after having been enlarged by me) an aperture of 3′ diameter, and the water is of a nearly constant temperature of 50° Fahr., although the air was, during my short stay, at 98°. The water is perfectly clear, and only disturbed by the continual evolution of carbonic acid gas, with a large quantity of which the water is saturated. This having been subjected by me to evaporation, a salt was obtained, which has been examined by Professor Daubeny of Oxford, who has given the results, which will be found at the conclusion of the present notice. The water has the taste of Seltzer water, and was examined shortly after my return from the Australian Alps, in the Civil Hospital of Sydney, by the Colonial Surgeon, and other qualified persons. Its chemical ingredients when ascertained by the application of the usual tests, very nearly correspond to those found by Prof. Daubeny in the salt.

I will mention a curious geological occurrence on the present occasion. The level of travertine before alluded to is covered with a white salt, efflorescing therefrom, and which, conjointly with the fragments of travertine strewed about, gives the whole locality the appearance as if some extensive buildings had been going on, and the plasterer just left off working. Caves, which contain bones, occur in the vicinity of the spring.

The following is the memorandum of Prof. Daubeny:

"The salt which you described as efflorescing near a chemical spring in Australia, contains carbonate of lime, common salt, a little sulphate of soda, and some peroxide of iron. The salt which you represented as being procured by the evaporation of the water, contains muriates, calcareous earth in combination with carbonic acid, a trace of magnesia, and iron."

I have a bottle of the water, which (like those examined in Sydney) was hermetically sealed at the mouth of the spring, to be kept for analysis.

XLVIII. *Proceedings of Learned Societies.*

GEOLOGICAL SOCIETY.

Jan. 9,—1839. A NOTICE was first read on the discovery of the Basilosaurus and the Batrachiosaurus, by Dr. Harlan.

The first remains of the Basilosaurus, which came under Dr. Harlan's notice, were a vertebra and some other bones found in the marly banks of Washeta river, Arkansas territory. In the autumn of 1834, he examined another collection discovered in a hard limestone in Alabama, and consisting of several enormous vertebræ, a humerus, portions of jaws with teeth, and some other fragments supposed to belong to the same animal. In the matrix of the vertebra from the Washeta river was a fossil Corbula, common in the Alabama tertiary deposits, and specimens of Nautilus, Scutella, and Modiolus of extinct and new species; sharks' teeth have also been found in a similar rock in the vicinity of the locality from which the other collection was procured. Dr. Harlan was originally inclined, from the structure of the teeth, to consider these fossil remains as having belonged to a marine carnivorous animal; but from an examination of the bones he was induced to conclude, that they were portions of a new genus of Saurians, for which he proposed the name of Basilosaurus.

Dr. Harlan then briefly described a portion of an upper jaw of a Saurian discovered by a beaver-trapper, on or near the banks of the Yellowstone river, in the territory of the Missouri, imbedded in a hard blue limestone rock. On first inspection Dr. Harlan believed, from the structure of the teeth, the mode of dentition, and the position of the *anterior nares*, the fragment belonged to an Ichthyosaurus; but as it differs entirely from that genus in having separate alveoli, and in the form and position of the intermaxillary bones, while it approaches in the latter characters the batrachian reptiles, he has formed for the fossil a new genus designated by the name of Batrachiosaurus.

A paper was afterwards read, entitled, "Observations on the Teeth of the Zeuglodon, Basilosaurus of Dr. Harlan," by Richard Owen, Esq., F.G.S., Hunterian Professor in the Royal College of Surgeons, London.

During the recent discussions respecting the Stonesfield fossil jaws, one of the strongest arguments adduced and reiterated by M. de Blainville and others in support of their saurian nature, was founded on the presumed existence in America of a fossil reptile possessing teeth with double fangs, and called by Dr. Harlan the Basilosaurus. To the validity of this argument, Mr. Owen refused to assent, until the teeth of the American fossil had been subjected to a re-examination with an especial view to their alleged mode of implantation in the jaw; and until they had been submitted to the

test of the microscopic investigation of their intimate structure with reference to the true affinities of the animal to which they belonged. The recent arrival of Dr. Harlan in England with the fossils, and the permission which he has liberally granted Mr. Owen of having the necessary sections made, have enabled him to determine the mammiferous nature of the fossil.

Among the parts of the Basilosaurus brought to England by Dr. Harlan, are two portions of bone belonging to the upper jaw; the larger of them contains three teeth; the other, the sockets of two teeth. In the larger specimen, the crowns of the teeth are more or less perfect, and they are compressed and conical, but with an obtuse apex. The longitudinal diameter of the middle, and most perfect one, is three inches, the transverse diameter one inch two lines, and the height above the alveolar process two inches and a half. The crown is transversely contracted in the middle, giving its horizontal section an hour-glass form; and the opposite wide longitudinal grooves which produce this shape, becoming deeper as the crown approaches the socket, at length meet and divide the root of the tooth into two separate fangs. The two teeth in the fore part of the jaw are smaller than the hinder tooth, and the anterior one appears to be of a simpler structure.

A worn-down tooth contained in another portion of jaw, Mr. Owen had sliced, and it presented the same hour-glass form, the crown being divided into two irregular, rounded lobes joined by a narrow isthmus or neck. The anterior lobe is placed obliquely, but the posterior parallel with the axis of the jaw. The isthmus increases in length as the tooth descends in the socket until the isthmus finally disappears, and the two portions of the tooth take on the character of separate fangs. It is evident that the pulp was originally simple, but that it soon divided into two parts, from which the growth of the ivory of the teeth proceeded as from two distinct centres, now separately surrounded by concentric striæ of growth, the exterior sending an acute-angled process into the isthmus. The cavitas pulpi, which is very small in the crown of the tooth, contracts as the crown descends, and is almost obliterated near the extremity, proving that the teeth were developed from a temporary pulp.

The sockets in the anterior fragment of the upper jaw are indistinct and filled with hard calcareous matter, but a transverse horizontal section of the alveolar margin proves, that these sockets are single, and that the teeth lodged therein had single fangs. In the anterior socket, there is an indication of the transverse median contraction, showing that this tooth resembled in form, to a certain degree, the posterior tooth. A plaster cast of a portion of the lower jaw afforded the only means of studying this part of the fossil. It contains four teeth, of which the two posterior are nearly contiguous, the next is at an interval of an inch and a half, and the most anterior of two inches from the preceding. The last tooth is more simple in form than those behind, and it has been described as a canine. This fragment of the lower jaw thus confirms the evidence afforded by the fragments of the upper jaw, that the teeth in the Basilosaurus

were of two kinds, the anterior being smaller and simpler in form, and further from each other than those behind.

Mr. Owen then proceeds to compare the Basilosaurus with those animals which have their teeth lodged in distinct sockets, as the Sphyræna, and its congeners among fishes, the Plesiosauroid and Crocodilean Sauria, and the class Mammalia; but as there is no instance of either fish or reptile having teeth implanted by two fangs in a double socket, he commences his comparison of the Basilosaurus with those Mammalia which most nearly resemble the fossil in other respects. Among the zoophagous Cetacea the teeth are always similar as to form and structure, and are invariably implanted in the socket by a broad and simple basis, and they never have two fangs. Among the herbivorous Cetacea however, the structure, form, number and mode of implantation of the teeth differ considerably. In the Manatee, the molars have two long and separate fangs lodged in deep sockets, and the anterior teeth, when worn down, present a form of the crown similar to that of the Basilosaurus, but the opposite indentations are not so deep; and the entire grinding surface of the molars of the Manatee differs considerably from those of the Basilosaurus, the anterior supporting two transverse conical ridges, and the posterior three. The Dugong resembles more nearly the fossil in its molar teeth; the anterior ones being smaller and simpler than the posterior, and the complication of the latter being due to exactly the same kind of modification as in the Basiloraurus, viz. a transverse constriction of the crown. The posterior molar has its longitudinal diameter increased, and its transverse section approaches to the hour-glass figure, produced by opposite grooves. There is in this tooth also a tendency to the formation of a double fang, and the establishment of two centres of radiation for the calcigerous tubes of the ivory, but the double fang is probably never completed. The teeth in the Dugong moreover are not scattered as in the Basilosaurus.

Mr. Owen then briefly compared the teeth of the fossil with those of the Saurians, and stated that he had not found a single instance of agreement in the Basilosaurus with the known dental peculiarities of that class. From the Mosasaurus the teeth of the American fossil differ in being implanted freely in sockets and not anchylosed to the substance of the jaw; from the Ichthyosaurus and all the lacertine Sauria in being implanted in distinct sockets, and not in a continuous groove; from the Plesiosaurus and crocodilean reptiles from the fangs not being simple and expanding as they descend, but double, diminishing in size as they sink in the socket, and becoming consolidated by the progressive deposition of dental substance from temporary pulp in progress of absorption. In the Enaliosauria and the Crocodilia, moreover, there are invariably two or more germs of new teeth in different stages of formation close to or contained within the cavity of the base of the protruded teeth; but the Basilosaurus presents no trace of this characteristic Saurian structure. From the external characters only of the teeth, Mr. Owen therefore infers, that the fossil was a Mammifer of the cetaceous

order, and intermediate to the herbivorous and piscivorous sections of that order, as it now stands in the Cuvierian system.

In consequence however of the Basilosaurus having been regarded as affording an exceptional example among Reptilia of teeth having two fangs, though contrary to all analogy, and as the other characters stated above may be considered by the same anatomists to be only exceptions, Mr. Owen procured sections of the teeth for microscopic examination of their intimate structure and for comparing it with that of the teeth of other animals.

In the Sphyræna and allied fossil fishes which are implanted in sockets, the teeth are characterized by a continuation of medullary canals, arranged in a beautifully reticulated manner, extending through the entire substance of the tooth, and affording innumerable centres of radiation to extremely fine calcigerous tubes.

In the Ichthyosaurus and Crocodile the pulp cavity is simple and central, as in Mammalia, and the calcigerous tubuli radiate from this centre to every part of the circumference of the tooth, to which they are generally at right angles. The crown of the tooth in these Saurians is covered with enamel, while that part of the tooth which is in the alveolus is surrounded with a thick layer of cortical substance. In the Dolphins which have simple conical teeth like the higher reptiles, the crown is also covered with enamel and the base with cæmentum. But in the Cachalot and Dugong the whole of the teeth is covered with cæmentum. In the Dugong this external layer presents the same characteristic radiated purkingian corpuscles or cells as in the cæmentum of the human teeth, and those of other animals; but the cæmentum of the Dugong differs from that of the Pachyderms and Ruminants in being traversed by numerous calcigerous tubes, the corpuscles or cells being scattered in the interstices of these tubes. Now the crowns of the teeth of the Basilosaurus evidently exhibit in many parts a thin investing layer of a substance distinct from the body or ivory of the tooth, and the microscopic examination of a thin layer of this substance proves it to possess the same characters as the cæmentum of the crown of the tooth of the Dugong. The purkingian cells are, in some places, scattered irregularly, but in others are arranged in parallel rows. The tubes radiating from the cells are wider than usual at the commencement; but soon divide and subdivide, forming rich reticulations in the interspaces, and communicating with the branches of the parallel larger tubes. These are placed, as in the Dugong, perpendicular to the surface of the tooth, but they are less regularly arranged than the calcigerous tubes of the ivory, with which, however, they form numerous continuations. There is a greater proportion of cæmentum in the isthmus of the tooth than elsewhere; and the worn-down crown of the tooth must therefore have exhibited a complicated structure. The entire substance of the ivory of the teeth consists of fine calcigerous tubes radiating from the centres of the two lobes, without any intermixture of coarser medullary tubes which characterize the teeth of the Iguanodon; or the slightest trace of the reticulated canals, which distinguish the texture of the teeth of the

Sphyræna and its congeners. The calcigerous tubes undulate regularly, and, like those of the Dugong, exhibit more plainly the primary dichotomous bifurcations, and the subordinate lateral branches given off at acute angles: they also communicate with numerous minute cells arranged in concentric lines.

Thus, the microscopic characters of the texture of the teeth of the great Basilosaurus are strictly of a mammiferous nature; and Mr. Owen further showed that they differ from those of the fossil Edentata, which are also surrounded by cæmentum, in the absence of the coarse central ivory; and confirm the inference respecting the position of the fossil in the natural system drawn from the external aspect of the teeth.

Mr. Owen then adduced further proofs of the mammiferous and cetaceous character of the Basilosaurus, from the structure of the vertebræ, which proves that the epiphyseal laminæ were originally separated from the body of the vertebræ, but were afterwards united to it. In the bodies of the smaller vertebræ the epiphyses are wanting, and Mr. Owen agrees with Dr. Harlan in inferring from the common occurrence of this condition, that there were originally three separate points of ossification in the body of the vertebræ; a character never noticed in the vertebræ of Saurians, but a most prominent one in those of the Cetacea. Another argument in favour of the mammiferous and cetaceous nature of the Basilosaurus is deduced from the great capacity of the canal for the spinal chord, which in the Cetacea is surrounded by an unusually thick plexiform stratum of both arteries and veins. The cetaceous character is further manifested in the short antero-posterior extent of the neurapophyses as compared with that of the body of the vertebræ; in their regular concave posterior margin, and the development of the articular apophyses only from their anterior part: also in the form and position of the transverse processes, which however present a greater vertical thickness than in the true Cetacea, and approach in this respect to the vertebræ of the Dugong.

With respect to the other bones of the Basilosaurus, Mr. Owen stated, that the ribs in their excentric laminated structure are peculiar, and unlike those of any Mammal or Saurian. The hollow structure of the lower jaw of the Basilosaurus, which has been advanced as a proof of its saurian nature, Mr. Owen showed occurs also in the lower jaw of the Cachalot, and is therefore equally good for the cetaceous character of the fossil.

In the compressed shaft of the humerus, and its proportion to the vertebræ, the Basilosaurus again approximates to the true Cetacea, as much as it recedes from the Enaliosaurians; but in the expansion of the distal extremity and the form of the articular surface, this humerus stands alone; and no one can contemplate the comparative feebleness of this, the principal bone of the anterior extremity, without agreeing with Dr. Harlan, that the tail must have been the main organ of locomotion.

Mr. Owen, in compliance with the suggestion of Dr. Harlan, who, having compared with Mr. Owen the microscopic structure of the

teeth of the Basilosaurus with those of the Dugong and other animals, admits the correctness of the inferences of its mammiferous nature, proposes to substitute for the name of Basilosaurus that of Zeuglodon, suggested by the form of the posterior molars, which resemble two teeth tied or yoked together.

A notice on "the Occurrence of Graptolites in the Slate of Galloway in Scotland," by C. Lyell, Esq., V.P.G.S., was first read.

On examining some specimens of slaty sandstone and shale, collected by Mr. John Carrick Moore, on the shore of Loch Ryan in Galloway, Mr. Lyell discovered distinct remains of Graptolites, resembling those found in the Silurian strata of England and Sweden. As Mr. Lyell is not aware of these zoophytes having been before observed in Scotland, and as organic remains are exceedingly rare in the great range of slaty sandstone and shale which extends from St. Abb's Head to Galloway, he considers the discovery of a fossil, affording a test of the relative age of those beds, not unimportant. The strata containing the Graptolites are nearly vertical, and their strike is west-south-west and east-north-east.

Mr. Sharpe's paper "On the Geology of the Neighbourhood of Lisbon," commenced at the meeting held on the 9th of January, was then concluded.

In 1832, Mr. Sharpe laid before the Society, a short account of the geological structure of the neighbourhood of Lisbon*; but having since that period resided for a considerable time in the same district, he gave in the paper read on the 23rd instant, the result of his more extended and matured acquaintance with the country.

The tract described by Mr. Sharpe, is bounded towards the north by a line extending from Torres Vedras by Sobral to Villa Franca, and in the south by the coast from Cape Espichel to St. Ubes; and the whole of its area is about 650 square miles.

The formations are arranged by the author in the following order, the local names having been taken from the points where the strata are best exhibited:

Tertiary. (*a.*) Upper tertiary sand.—(*b.*) Almada beds.—(*c.*) Lower tertiary conglomerate.

Secondary. (*d.*) Hippurite limestone.—(*e.*) Red sandstone.—(*f.*) Espichel limestone.—(*g.*) Slate clay and shale.—(*h.*) San Pedro limestone.—(*i.*) Older red conglomerate.

Igneous Rocks.—Basalt.—Granite.

TERTIARY FORMATIONS.

The tertiary deposits occupy a tract, only a portion of which is included within Mr. Sharpe's district, as they extend in a north-east direction to Abrantes, a distance of eighty miles, and in a south-east to Alcacer do Sal, a distance of fifty miles. The Tagus flows through the tract from Abrantes to the sea, but the greater part of the tertiary strata are situated to the south of the river.

(*a.*) *Upper Tertiary Sand.*—This formation consists of about 100 feet of fine gray quartzose sand, and 150 feet of coarse quartzose ferruginous sand and gravel. It constitutes nearly the whole of the

* Proceedings, vol. i. p. 394. [or L. & E. Phil. Mag., vol. i. p. 227.—Ed.]

X 2

tertiary district, south of the Tagus, included within the author's survey. The strata are usually quite horizontal, except at the edges of the basin, where they rest upon the inclined beds of the subjacent deposits; and the author did not observe any instance of their having been disturbed. They generally repose upon the Almada limestone, but near Aldea do Meco, to the north of Cape Espichel, they are in contact with the red sandstone formation. No traces of organic remains have been noticed in any part of these sands. In the lower beds a mine of quicksilver was worked profitably during the last century near Coina, south of the Tagus; and the gold dust for which the sands of that river have been so long celebrated, Mr. Sharpe believes, is derived also from the lower or ferruginous sands.

(*b.*) *Almada Beds.*—A complete section of this deposit is not exhibited in the neighbourhood of Lisbon, and the strata are so very irregular both in thickness and composition, that it is difficult to connect the sections displayed at different localities. The strata are best exposed in the cliff south of the Tagus, between Trafaria and Almada. The whole of the series is arranged by Mr. Sharpe in three groups, the uppermost consisting of limestone and sands, the middle of blue clay, and the lowest of another series of limestones and sands: but Mr. Sharpe does not attach much value to the subdivision; as the same fossils are found in the beds above and below the blue clay. The deposit constitutes a triangular tract on the Lisbon side of the Tagus, extending from that city to Verdelha, a distance of about fourteen miles; it also caps some hills between Belem and Fort St. Julian. South of the Tagus, it forms the cliffs already mentioned; and a band which ranges from St. Ubes northwards to Palmella, and thence south-west to within a mile of Aldea do Meco, skirting the flanks of a ridge of secondary formations. A detached mass of the Almada beds occurs at the western end of the Serra de San Luiz, between St. Ubes and Azeitão, abutting unconformably against the elevated edges of the beds of red sandstone, and another is on the shore at the foot of San Felippa near St. Ubes. North of the Tagus, the beds incline from 5° to 10° to the south-east; but to the south of the river between St. Ubes and Aldea do Meco, the dip varies from 25° to 30°, and conforms to the position of the band with respect to the ridge of secondary rocks, being to the south-east between St. Ubes and Palmella, and to the north-west between the latter town and Azeitão. The beds of the detached mass near the western end of the Serra de San Luiz, dip about 30° north, and those of the mass on the shore at the foot of San Felippa, 80° towards the older red conglomerate, having been thrown over beyond the perpendicular. On the coast at Casilhas near Almada, the level of the strata is affected very considerably by faults. North of the Tagus a fault cuts off the tertiary strata at Oeiras, the Almada beds forming one bank of the stream, and the Hippurite limestone the opposite; but the strata of each deposit are horizontal. In Lisbon the Almada beds rest unconformably on the Hippurite limestone; but between the city and Verdelha, conformably on the lower tertiary conglomerate. In the band ranging from St. Ubes by Palmella towards Aldea do Meco, they

repose in general also conformably on the red sandstone. The greatest height attained by the formation is the Castle Hill near Palmella, the summit of which is 930 feet above the level of the sea, and at this point two lines of disturbance meet. Fossils are very abundant in some of the beds, but sufficient attention has not yet been paid to them to permit their being compared with the organic remains of other tertiary districts. A long-hinged oyster, *Ostrea longirostris*, Mr. Sharpe considers identical with a species common in the tertiary deposits of the south of Spain. Small quantities of quicksilver have been found in several places in a bed of sand immediately above the blue clay or central division of the formation.

(*c.*) *Lower Tertiary Conglomerate.*—This deposit consists in the upper part of distinctly stratified conglomerates, composed of limestone pebbles imbedded in a calcareous matrix; and in the lower of sands, grits, gravel, and marl. Within the district examined by Mr. Sharpe, it occurs only on the Lisbon side of the Tagus, forming a band from that city by Odivellas, Camarate, Loures, and Tojal, to the neighbourhood of Alhandra, on the banks of the Tagus, and skirting the western and north-western boundary of the Almada beds. The conglomerate occurs also on some of the detached hills between Belem and the Bay of Cascaes. The deposit dips to the south-east under the Almada beds at an angle of 10° or 15°, but in the lowest strata the dip is 30°. For a short distance south of Alhandra, the conglomerate rests upon the red sandstone, but throughout the remainder of its range upon basalt. No organic remains were noticed in the deposit.

SECONDARY FORMATIONS.

In few countries can the separation between the tertiary and secondary formations be more strongly marked than in the neighbourhood of Lisbon. The deposits of the older class of rocks, Mr. Sharpe states, were disturbed and denuded previously to the commencement of the tertiary epoch, and an immense mass of basalt is interposed between the newest of the secondary rocks and the most ancient of the tertiary series.

(*d.*) *Hippurite Limestone.*—The upper part of this formation consists of alternations of marl and limestone, succeeded by beds of limestone containing thin horizontal beds of flint; and the lowest part of various strata of compact limestone; amounting in the whole to a thickness of above 500 feet. The formation is confined to the north of the Tagus, where it presents several distinct bands, which rest upon the red sandstone, and are overlaid by basalt. The most southern tract extends from Cascaes Bay nearly to Loures; another irregular strip ranges from Montelavar to a little to the eastward of Bucellas; and a third district, commencing near Villa Franca, stretches to the north beyond the range of Mr. Sharpe's district. A portion of Lisbon also stands upon Hippurite limestone. In some parts, especially on the coast, the dip is slightly towards the south-east, but from Loures to beyond Bellas it varies from 30° to 50° in the same direction. The strata do not always rest conformably on those of the subsequent red sandstone, for near Cascaes, the limestone beds are horizontal, and the sandstone on which

they lie is inclined at a considerable angle. The narrow valley of Alcantara, close to Lisbon, is the line of a considerable fault, the strata dipping in opposite directions from the valley, or 15° towards the west, and 10° towards the east. Another anteclinal line intersects the upper part of this valley; and at the point where the two disturbances cross, considerable derangement of the strata is produced. In one quarry Mr. Sharpe noticed eight small faults, and the walls of the rocks on each side of the fissures had a beautiful polish. Though the author has adopted the term Hippurite limestone for this deposit, yet he did not discover any remains of that genus, but great abundance of Spherulites, some of them probably of known species, and other fossils of the family of Rudista. He obtained also a considerable number of shells, including *Exogyra flabellata, Pecten quadricostatus* and *Pecten striato-costatus*.

(*e.*) *Red Sandstone.*—This formation consists of various sands, sandstones, marls, and limestone, which are grouped by Mr. Sharpe in the following manner:

Upper Division.—Ferruginous sands, sandstones, and coloured marls.

Middle Division.—Calcareous sandstones and coarse limestones.

Lowest Division.—Coarse sands, sandstones, and grits.

The extent of country, composed of this formation, is very considerable. North of the Tagus, the red sandstone covers the greater portion of the area to the westward of the tertiary strata and Hippurite limestone, the only tract belonging to other deposits being the hills at Cintra, and the lower ridges immediately surrounding them. A denuded strip of sandstone is also exposed between Loures and Cape Sinchette. South of the Tagus, the red sandstone forms a tract of variable breadth, extending from Palmella to the coast, a little north of Cape Espichel. The beds of this formation are greatly affected by faults and vary much in the angle of inclination, but the prevailing dip is towards the south-east throughout the districts on the Lisbon side of the Tagus. In the tract between Palmella and the coast, the strata have also been disturbed by considerable faults, but their usual dip is north, or north-west, at a high angle. Near Lisbon, the connexion of the red sandstone with the subjacent formations is not often exposed. North of Cintra the sandstone rests almost horizontally upon inclined strata of Espichel limestone, shale, San Pedro limestone and granite. South of the Cintra hills, it reposes very irregularly upon the Espichel limestone: and south of the Tagus, with every degree of want of conformity, upon the limestone of the Serra d'Arrabida (Espichel limestone); and in a great variety of positions upon the lofty peaks of the older red conglomerate of the Cavoens and the Serra de San Luiz near St. Ubes. Lignite occurs in several places, and in sufficient quantities to have led to unsuccessful researches for coal. Sulphur also thickly encrusts some of the sandstone strata; and gypsum has been worked near Santa Anna, south of the Tagus. Mr. Sharpe is of opinion, that the tepid springs of Estoril, near Cascaes, may derive their virtues from the sulphureous strata; and that the hot springs of Caldas da Rainha may owe their sulphureous qualities to similar strata.

The only organic remains found in the sandstones, are vegetable impressions and seed-vessels; but in the calcareous beds, corals and shells occur, and Mr. Sharpe has been able to identify some of the latter with the *Perna rugosa, Trigonia literata* and *Terebratula intermedia*, of the English secondary oolitic series.

(*f.*) *Espichel Limestone.*—This formation constitutes the flat, outer band which encircles the Cintra hills, also the range of hills between Cape Espichel and Cezimbra, and most probably the Serra d'Arrabida near St. Ubes. At the first of these localities, it consists of thick beds of gray coarse limestone, alternating with thinner ones of shale or marl; at the second, of a similar limestone with fewer layers of shale; and at the Serra d'Arrabida, of compact gray limestone with no partings of shale, except towards the bottom of the formation. Around the hills of Cintra, the strata dip as from a centre, at angles varying from 20° to 75°; between Cape Espichel and Cezimbra their inclination is from 45° to 70° to the north; and in the Serra d'Arrabida the prevailing dip is also to the north at a high angle, but at the west end of the Serra it varies from north to north-west and north-east; whilst in the northern side of the Serra de Vizo, or the eastern prolongation of the Serra d'Arrabida, the dip is toward the south. In the Cintra district the limestone rests conformably on the subjacent formation of shale; between Cape Espichel and Cezimbra, and in the Serra d'Arrabida the bottom beds are not exposed, and consequently the connexion with the inferior deposits is not visible; but in the Serra de Vizo the limestone reposes quite unconformably upon highly inclined strata of the older red conglomerate. The organic remains of this formation are principally casts of shells, which are not easily separable from the matrix. One of the specimens obtained by Mr. Sharpe closely resembles a Trigonia from the green sand of Blackdown.

(*g.*) *Shale.*—The upper portion of this deposit consists principally of shale, varying a good deal in character; the middle of indurated shale alternating regularly and conformably with beds of trap from five to twenty feet thick, and the lowest of dark shale. Near Ramalhão, where the formation is best displayed, there are from twenty to thirty distinct alternations of igneous rocks and shale, the latter being altered and indurated; but in the cliff at the Praia de Adraga, where the deposit is diminished to about 200 feet, there is only one bed of igneous origin. The formation rests with perfect conformity on the San Pedro limestone, dipping on all sides from the central granite axis of Cintra, at angles from 30° to 60°.

(*h.*) *The San Pedro Limestone* forms an inner zone around the Cintra hills, resting upon the granite. The upper beds are dark gray and earthy; but as the limestone approaches the granite, it gradually passes into a crystalline marble. At the village of San Pedro the following series is exposed:—

Dark gray compact limestone several hundred feet thick.	
Gray limestone with very slight traces of crystalline texture, and towards the bottom granular......	200 feet
Coarse crystalline marble, white or gray and white	100

Coarser crystalline marble, usually gray, but towards the bottom bluish white, and still coarser ...... 100 feet.
Granite.

The same gradual change may be traced all around the Cintra hills, wherever the limestone can be seen resting upon or approaching the granite. The lines of stratification are scarcely affected by the change in the structure of the stone, and the dip is from the granite at angles between 40° and 70°. Imperfect casts of a bivalve and an univalve were found in this limestone by the author.

(*i.*) *Older Red Conglomerate.*—This formation occurs only west of St. Ubes; and though Mr. Sharpe describes it the last of the sedimentary series, yet he is not certain respecting its relative geological antiquity. Near St. Ubes it rises from beneath the red sandstone and the Espichel limestone, and it is therefore older than either of those rocks. The conglomerate consists of rounded pebbles of white or ferruginous quartz, with a few of jasper, mica slate, and limestone. They vary from half an inch to more than a foot in diameter, and are firmly embedded in a coarse ferruginous sandstone. The highest ridge of the Serra de Covoens consists of this formation, also the eastern end of the Serra de San Luiz, the higher parts of the Serra de Vigo, and the coast from St. Ubes to the foot of the Serra d' Arrabida. At the eastern end of the Serra de Covoens and in the Serra de San Luiz, the dip of the beds is to the north, at angles varying from 30° to 50°; at the eastern end of the Serra de Vigo they incline about 30° to the south; more to the westward, in the same serra, they are in some places vertical, in others they dip about 50° to the north; and at the Torre de Outão, at the foot of the Serra d' Arrabida, they are inclined about 70° north-east.

The description of the sedimentary rocks is followed by an attempt to compare each formation with its probable equivalent in other parts of Europe; but as the Lisbon fossils have not yet been examined with sufficient care, Mr. Sharpe does not venture to draw any positive conclusions.

Of the tertiary series, the Almada beds alone offer any terms of comparison, and these are not very satisfactory. The fossils collected by the author are said to differ from those of the London clay, with the exception of one species, which is considered identical with *Natica similis*; but a long-hinged oyster, *Ostrea longirostris*, abundant in the Almada beds, agrees with a fossil common in the tertiary strata of Baza, Lorca and Alhama, in the south of Spain, described by Brigadier Silvertop; and Mr. Sharpe from an examination of these deposits, as well as from the agreement in the oyster, is induced to consider the Murcia and the Lisbon series as of the same age.

The Hippurite limestone, Mr. Sharpe has no doubt, is the equivalent of the extensive formation in the south of Europe characterized by the abundance of remains belonging to the family of *Rudista*, and considered the representative of the chalk and greensand series of England and the north of France.

The red sandstone Mr. Sharpe considers to belong also to the secondary system, in consequence of his having obtained from it

specimens of *Terebratula intermedia, Perna rugosa,* and *Trigonia literata.*

Of the formations below the red sandstone, the author offers no data for establishing a comparison with deposits in other parts of Europe further than that the Espichel and Arrabida limestones may be of the same age as the limestone of the rock of Gibraltar, and that the shale near Cintra may be the equivalent of the shale which underlies the Gibraltar limestone, and constitutes a considerable portion of Andalusia. He is also of opinion, that the Cintra shale is of the same age with the immense deposit of similar composition, which covers the centre of the province of Alentejo, extending from Alcacer do Sol to the confines of Algarve.

The older red conglomerate of the neighbourhood of St. Ubes, Mr. Sharpe considers as probably identical with the conglomerate largely developed on the banks of the Vonga, and which rests upon mica slate a little to the south of Oporto.

IGNEOUS ROCKS.

Basalt.—The principal deposit of this rock forms one of the most important features in the geology of the district to the north and west of Lisbon, occupying an irregular area, estimated to be not less than eighty square miles. It is difficult to define its limits without reference to an accurate map; but it may be stated to form a tract of very varying breadth, from the shore west of Belem by Queluz, and Odivellas to Loures. In the neighbourhood of the last village, it turns S.W. and N.E., ranging in the former direction to the neighbourhood of Montelavar, and in the latter nearly to Verdelha on the banks of the Tagus. Besides this immense continuous mass, many of the hills north of Oeiras, near the mouth of the Tagus, are capped by basalt, evidently outlying patches, once connected with the great deposit. Basalt also forms the summit of the hills near Sobral and St. Sebastiano, resting upon the red sandstone. It has been already stated, that beds of trap alternate regularly and without any appearance of disturbance with the central division of the shale formation near Cintra.

The rock varies considerably in character, and is occasionally columnar. It is stated to have frequently the appearance of a black indurated clay with an irregular schistose cleavage, and breaking into very irregular rhombs.

The only beds which rest upon the basalt belong to the tertiary series, but it overlies both the Hippurite limestone and the red sandstone. To the westward of Loures, it cuts through these formations; and the red sandstone, to the south of the line of intersection, has been brought to a level with the Hippurite limestone to the north of the line. The strata of Hippurite limestone to the north are nearly horizontal, while those of the red sandstone, and limestone to the south, are highly inclined. Hence Mr. Sharpe infers that the great mass of basalt was poured forth from fissures in the neighbourhood of Loures.

The cliffs in the bay of Cascaes exhibit fine sections of basaltic dykes and disturbances; and on the beach west of Cezimbra masses of basalt are intruded into strata of red sandstone, which exhibit

great marks of disturbance. The Espichel limestone and the red sandstone have been also greatly elevated at the Castle Hill at Cezimbra, by a trap rock of which the date is uncertain.

Although the author had innumerable opportunities of observing the junction of the basalt with the beds below it, yet in no instance did he observe any change in the characters of the subjacent rocks. The alteration produced in the beds of shale, which alternate with trap rocks near Cintra, has been already noticed. Mr. Sharpe considers these igneous strata to have been ejected contemporaneously with the deposition of the shale, and to be consequently older than the great coating of basalt. The Espichel limestone, in contact with the trap near Cezimbra, is also altered, being of a crystalline texture to a distance of fifty feet from the igneous rock.

Granite is found only in the neighbourhood of Cintra, forming a range of hills about seven miles in length and five in breadth. Their greatest altitude is less than 2000 feet. The prevailing rock is a true granite consisting of nearly equal proportions of quartz and felspar with a little mica; but towards the western end of the chain, syenite and porphyry occur. In the central portions of the hills, the granite is coarsely grained, and splits into large irregular blocks; but on the flanks it is schistose, finely grained, cleaves into rhombs, and might be mistaken for a sandstone. Veins of large-grained granite, however, occur in the schistose variety, and veins of finely-grained in the coarse central masses.

Mr. Sharpe then describes, in detail, the dislocations in the sedimentary strata on the flanks of the granitic hills; and he shows that all the formations, from the San Pedro limestone to the Espichel, have been dislocated, and thrown into highly inclined positions, but the details cannot be clearly understood without the aid of sections. It may however be stated, that in consequence of the red sandstone resting in nearly horizontal strata against the inclined beds of the lower formation, the latter was disturbed previously to the deposition of the sandstone, and that consequently the irruption of the granite of Cintra took place at a period anterior to the origin of the sandstone.

Mr. Sharpe describes also with considerable minuteness the disturbance near Palmella, south of the Tagus; and he infers, from the relative position of the strata, that there have been, in that district, considerable elevations at four distinct periods.

The paper concludes with some observations on the earthquake of 1755; and the author, shows, that its effects were entirely confined to the tertiary strata, and were most violently felt on the blue clay belonging to the Almada beds, on which the lower part of the city is constructed. Not a building on the Hippurite limestone, or the basalt, was injured.

ASTRONOMICAL SOCIETY.

Jan. 11, 1839.—The following communications were read: "On the Obliquity of the Ecliptic." By the Rev. Dr. Pearson.

The object of this paper is to give an account of a determination of the annual diminution of the obliquity, by a comparison of obser-

vations of the solstices made by Dr. Pearson at South Kilworth, with similar observations of Bradley at the Royal Observatory. Dr. Pearson remarks, that the most remote determination of the obliquity which has any claim to precision, is Dr. Bradley's; and, accordingly, some one of his obliquities has been chosen by almost all practical astronomers for the purpose of comparison with their own, in order to determine the annual diminution. But, notwithstanding the apparent facility with which the diminution may be obtained, by a comparison of determinations separated by a considerable interval of time, no two astronomers agree in their results. Dr. Maskelyne considered it to be $-0''\cdot52$; Delambre adopted $-0''\cdot48$; Dr. Brinkley, $-0''\cdot43$; and Bessel, $-0''\cdot457$. These discrepancies, which in the course of years amount to considerable quantities, demand that the question of preference should be settled; and this can only be effected by practical methods.

Bradley determined his summer and winter obliquities by separate deductions,—a method which rendered the result dependent on the assumed latitude of the place. He assumed the latitude of the observatory to be $51° 28' 40''$, which has since been shown to be too great by at least one, if not two, seconds; and, accordingly, all his obliquities are affected with a corresponding error, which may explain the reason why the Greenwich winter obliquities are smaller by some seconds than the summer ones. But it is easy to see that, by combining the observations of two successive solstices, the latitude may be eliminated; for, half the difference of the sun's extreme meridian altitudes gives the obliquity due to the middle of the interval, which, if the winter solstice be taken first, will be the vernal equinox. In like manner, if we take three, or any *odd* number of successive obliquities, separately deduced by means of an assumed colatitude, the sum of the whole, divided by their number, will give the mean obliquity belonging to the middle epoch, independent of the assumed colatitude.

Bradley's first recorded determination of an obliquity was in the winter of 1753; and he observed seven winter solstices, and as many summer ones, without interruption. Omitting the first determination, in order to have an odd number, and taking the thirteenth part of the sum of the remaining thirteen half-yearly determinations, we have an average obliquity corresponding to June 1757, viz. $23° 28' 13''\cdot4446$.

Dr. Pearson commenced his solstitial observations in June 1828, and continued them till June 1838, thereby obtaining twenty-one successive half-yearly obliquities, the sum of which gives an average obliquity corresponding to June 1833. His result is $23° 27' 39''\cdot2409$, and is therefore less than the average resulting from Bradley's determinations by $34''\cdot2037$. Dividing this difference by 76, the number of years between the two epochs (1757—1833), the annual diminution is found $= -0''\cdot4500$. This accords very nearly with the annual diminution adopted by Bessel in the Tabulæ Regiomontanæ.

The instrument with which the observations were made, is an al-

titude and azimuth instrument, described circumstantially in vol. ii. part i. of the Memoirs. Dr. Pearson describes the mode in which the instrument was used and its errors corrected, together with the methods followed in reducing the observations, and the elements employed in computing the corrections for parallax, refraction, nutation, and the sun's latitude; and concludes with a synopsis of the reduced observations, which were in number 1648, and a table of the mean obliquity on the 1st of January in each year, from 1750 to 1900, both inclusive, deduced from the above determination of the annual diminution.

"On the Parallax of α *Centauri*." By Professor Henderson.

The two stars designated α^1 and α^2 *Centauri*, are situated within $19''$ of space of each other. On comparing the observations of Lacaille with those of the present time, it has been found that, although the two stars have not sensibly changed their relative positions, each has an annual proper motion of 3·6 seconds of space. It thus appears that they form a binary system, having one of the greatest proper motions that have been observed; and from this circumstance, and the brightness of the stars, it is reasonable to suppose that their parallax may be sufficiently sensible to powerful instruments.

On reducing the declinations from his observations at the Cape of Good Hope, Mr. Henderson remarks, that a sensible parallax appeared, but he delayed communicating the result until it should be seen whether it was confirmed by the observations of right ascension made by Lieutenant Meadows with the transit instrument. He now finds that these observations also indicate a sensible parallax.

It is to be observed, that the observations both of right ascension and of declination were not made for the purpose of ascertaining the parallax, but of determining the mean places of the stars with a proper degree of accuracy. Had the author been aware of the proper motion at an earlier period, a much greater number of observations, and of such as would have been better adapted for ascertaining the parallax, would have been made, and the result thereby rendered more secure.

The right ascensions and declinations of the two stars (which are always above the horizon of the Cape, and favourably placed for observation at all seasons,) have been determined by comparisons with such of the principal or standard stars as were observed on the same day. It is consequently assumed that the latter have no sensible parallax. The mean places of the standard stars, or rather their relative positions, are also assumed to be known; and, in reducing the observations to the beginning of 1833, the coefficient of aberration has been assumed $=20''\cdot5$, and that of lunar nutation $=9''\cdot25$. Recent observations make the coefficient of aberration less; but a term is introduced into the equations of condition, by which the effect of a change in the aberration is immediately obtained.

For the determination of the parallaxes, three systems of equations of condition are formed for each star, namely, from the observations of right ascension, the direct observations of declination,

and the reflected observations of declination. On resolving the equations by the method of least squares, and assuming the coefficient of aberration to be $20''\cdot36$, Mr. Henderson finds the following results:

Parallax of α^1 *Centauri* =

$+0''\cdot92$; probable error $0''\cdot35$; from observations of right ascension.
$+1\ \cdot42$; probable error $0\ \cdot19$; from direct observations of declination.
$+1\ \cdot96$; probable error $0\ \cdot47$; from reflected observations of declination.

And $= +\ 1''\cdot38$, with a probable error of $0''\cdot16$, by taking a mean of the three determinations according to their weight.

Parallax of α^2 *Centauri* =

$+0''\cdot48$; probable error $0''\cdot34$; from observations of right ascension.
$+1\ \cdot05$; probable error $0\ \cdot18$; from direct observations of declination.
$+1\ \cdot21$; probable error $0\ \cdot64$; from reflected observations of declination.

And $= +\ 0''\cdot94$, with a probable error of $0''\cdot16$, by taking the mean according to their weights.

If we suppose that the two stars are at the same distance, then the parallax $= +\ 1''\cdot16$, with a problem error of $0''\cdot11$. It therefore appears probable, that these stars have a sensible parallax of about one second of space.

XLIX. *Intelligence and Miscellaneous Articles.*

POSTSCRIPT TO THE COMMUNICATION OF PROF. SEDGWICK AND MR. MURCHISON IN THE PRESENT NUMBER AT P. 241.

We alluded to the possibility of the Devonian or Old Red System being much developed in Ireland, and under mineral characters analogous to those of Devon. In support of this view, we may add, that since this paper was printed, Mr. Charles W. Hamilton has called our attention to a paper read by him before the Geological Society of Dublin, in 1837, in which he described the rock hitherto called grauwacke slate, which occupies a great extent in the county of Cork, as lying conformably upon the old red sandstone and conglomerate of the Gaulty Mountains and the Reeks in Kerry, and supporting the carboniferous limestone into which it gradually passes.

This red conglomerate rests unconformably upon a great thickness of older slates and conglomerates, which Mr. Hamilton has described as exactly similar to those which are exposed in a section made from Mr. Pennant's quarries in Caernarvonshire to Ogwen. In a paper read before the Geological Society of Dublin last month, Mr. Hamilton has also stated his belief, that slates occupying a large space between the Mourne and Dublin Mountains, are equivalent to those in the immediate neighbourhood of Cork, and may therefore be referred to the old red sandstone.

MR. CROSLEY'S PNEUMATIC TELEGRAPH.

In inserting the account of Mr. Crosley's pneumatic telegraph in our last number, we had not space for some of the details, which we therefore now give in continuation.

In establishments where the telegraphic communications do not require the constant attendance of a person to observe them, and

where periodical attendance is sufficient, the signals may be correctly registered on paper, by connecting with the air-tube an instrument called a *pressure register*, invented by the projector of the *pneumatic telegraph*, which has been successfully employed in large gas-light establishments upwards of fourteen years, for registering the variations of the pressure of gas in street mains. The same instrument produces also an increased range of the index scale, by which means the chance of errors from *minute divisions* is obviated.

It may be observed, that the introduction of railways has not only created an additional use for telegraphic communications, but the important difficulty which previously existed in the expense of providing a proper line and safe foundation is, at once, removed by the site of the railway itself, possessing as it does, by its police, the most ample security against injury, either to the tubes or electric wires.

There being now three different projects for improvements in telegraphic communications, viz. the electro-magnetic, the hydraulic, and the pneumatic telegraph,—and assuming that such improvements are of importance to the state, as well as to railway proprietors and the community at large, it seems desirable that their merits should be thoroughly investigated by competent engineers, and that the joint aid of Government should be solicited, in conjunction with that of railway proprietors, for the purpose of establishing, on a practical scale, the most eligible project.

The prominent questions for consideration seem to be—the certainty and accuracy of the communications, the first cost, the expense of repair and superintendence, also the time required for transmitting intelligence.

On the question of time, it is quite clear that neither the hydraulic nor the pneumatic can compete with the electro-magnetic telegraph in rapidity. No doubt, on investigation, each project will be found to possess its peculiar advantages. Thus, in considering the advantage one may have in point of time, another may possess a greater degree of certainty or accuracy in the communications, sufficient to outweigh the difference of time, for instance, between 1 second and 1 minute, or even between 1 second and 5 or 10 minutes.

The time occupied in transmitting intelligence by the pneumatic telegraph will depend on the capacity of the air-tube, the degree of compression given, and the distance between the stations; but should greater dispatch be required than is afforded by one tube, and the cost be of minor importance, several tubes may be employed, each fitted in the manner above described, so that all the figures contained in one telegraphic number, may be communicated at once with four tubes. Nine thousand nine hundred and ninety-nine different signal numbers may be communicated, referring to so many words or sentences, and these numbers may be multiplied fourfold by letters A, B, C, &c., as indices to distinguish each series.

The projector of the *pneumatic telegraph* is not in possession of any results of experiments on a practical scale by the electro-magnetic or by the hydraulic telegraph, employed at any considerably extended distances, or of their continued operation for any long period of time; nor can he offer much decisive information, of a

practical nature, analogous to the operation of the pneumatic telegraph on these points; the following circumstances may, however, be referred to:

There has been upwards of twenty years' experience in the transmission of gas for illumination through conduit pipes of various dimensions. In several instances, the gas has been supplied at the distances of from five to eight miles by low degrees of pressure. As one proof of great rapidity of motion, it has been observed, that when any sudden interruption in the supply has occurred at the works, the extinction of all the lights, over large districts, has been nearly simultaneous. Another instance of the great susceptibility of motion which frequently happens, is the flickering motion of the lights at great distances when water has accumulated in the pipes.

The only experience in the transmission of atmospheric air through conduit tubes, which applies more particularly to this question, may be referred to at three railway establishments; viz. Edinburgh, Liverpool, and Euston-square, London. In these establishments, air-tubes, from $1\frac{1}{4}$ to 2 miles in length, have been employed for the purpose of giving notice when a train of carriages is ready to be drawn up the inclined plane by the stationary engine at the summit, so that it may without delay be put in motion. This notice is communicated by blowing a current of air through the tube at the foot of the inclined plane, and sounding an organ-pipe, a whistle, or an alarm-bell at the stationary engine. It will be satisfactory to know, that this operation has been regularly performed from two to four years without one single failure or disappointment.

METEOROLOGICAL OBSERVATIONS FOR FEBRUARY, 1839.

Chiswick.—Feb. 1. Overcast: fine: frosty at night. 2. Sharp frost. 3. Thawing: hazy. 4. Fine: cloudy. 5. Hazy: heavy rain at night. 6. Foggy. 7. Drizzly. 8. Hazy: cloudy and windy at night. 9. Overcast. 10. Very fine. 11. Dense fog. 12. Fine: overcast: rain. 13. Fine. 14. Boisterous. 15. Clear. 16. Stormy and wet: fine. 17. Clear. 18. Snowing: sleet: clear. 19. Sharp frost. 20. Bleak and cold. 21. Cloudy and cold: dry haze: rain at night. 22. Hazy: rain. 23. Rain: very fine. 24. Very fine. 25. Clear: showery: fine. 26. Clear and frosty: fine. 27. Fine. 28. Very fine.

Boston.—Feb. 1. Fine. 2. Cloudy. 3. Cloudy: rain early A.M. 4, 5. Fine: rain P.M. 6. Cloudy. 7, 8. Fine. 9. Fine: rain P.M. 10. Fine. 11. Cloudy. 12, 13. Fine. 14. Stormy. 15. Fine. 16. Cloudy: rain early A.M. 17—19. Fine. 20, 21. Cloudy. 22. Cloudy: snow early A.M. 23. Cloudy: rain early A.M. 24. Fine. 25. Fine: hail and snow P.M. 26. Fine. 27. Cloudy. 28. Fine.

Applegarth Manse, Dumfries-shire.—Feb. 1. Clear day: ground covered with snow. 2. Cloudy: gentle thaw. 3. Moderate thaw: snow melting slowly. 4. Moderate thaw: small rain evening. 5. Thaw continuing: snow melting. 6. The same: very temperate: rain P.M. 7. Stormy day: snow gone: very wet. 8. Quiet A.M.: wind rose P.M.: wet. 9. Rain: dark and cloudy: mild. 10. Fair and mild: threatening P.M. 11. Raw cold: cloudy. 12. Fine day: flying hail showers. 13. Mild A.M.: rain and wind P.M. 14. Boisterous day: frequent hail and sleet. 15. Tolerable spring day: wet P.M. 16. Showers of snow: high wind. 17. Snow half an inch deep: frosty. 18. Moderate day: snow melting: freezing. 19. Fine frosty day: getting cloudy P.M. 20. Favourable day: slight snow. 21. Hard frost: cloudy: slight snow P.M. 22. Thaw: snow preceding night: snow melts. 23. Very fine day: temperate and spring-like. 24. On the whole mild: occasional showers. 25. Moderately temperate: slight frost A.M. 26. Fine day though rather chill. 27. Severe showers of sleet: cleared up P.M. 28. Occasional slight showers.

Meteorological Observations made at the Apartments of the Royal Society by the Assistant Secretary, Mr. Roberton; *by* Mr. Thompson *at the Garden of the Horticultural Society at Chiswick, near London; by* Mr. Veall *at Boston, and by* Mr. Dunbar *at Applegarth Manse, Dumfries-shire.*

Days of Month. 1839. Feb.	Barometer.						Thermometer.								Wind.				Rain.				Dew-point.
	London: Roy. Soc. 9 a.m.	Chiswick.		Boston. 8½ a.m.	Dumfries-shire.		London: Roy. Soc.			Chiswick.		Boston. 8½ a.m.	Dumfries-shire.		London: Roy. Soc. 9 a.m.	Chiswick 1 p.m.	Bost.	Dumfries-shire.	London: Roy. Soc. 9 a.m.	Chiswick.	Boston.	Dumfries-shire.	Lond.: Roy. Soc. 9 a.m.
		Max.	Min.		9 a.m.	8½ p.m.	Fahr. 9 a.m.	Self-register. Max.	Self-register. Min.	Max.	Min.		9 a.m.	9 p.m.									
1.	30·008	30·121	30·067	29·73	30·17	30·13	29·7	33·8	26·8	36	20	27	23	24	NW.	N.	calm	NW.	·050	...	...	...	25
2.	30·076	30·130	30·101	29·70	30·05	29·90	32·5	35·3	30·0	41	29	31·5	28	35	NW.	NW.	calm	SW.	·038	...	...	...	26
3.	29·868	29·910	29·875	29·50	29·82	29·78	37·4	38·3	32·7	45	29	35	32½	34½	S.	SW.	W.	SW.	·038	·42	·09	...	30
4.	29·822	29·866	29·850	29·44	29·70	29·58	38·7	39·3	36·6	49	33	33	33	40	SE.	S.	calm	SE.	...	·12	...	...	32
5.	30·104	30·165	29·974	29·69	29·97	29·98	41·8	48·8	38·3	46	36	37	36	34½	W.	S.	calm	SE.	·088	·39	·10	1·05	34
☾ 6.	30·122	30·232	30·119	29·81	30·12	29·84	39·3	45·3	40·0	50	42	39	35	39	N.	SE.	calm	SE.	·377	...	·12	...	35
7.	30·288	30·336	30·298	29·80	30·11	29·86	47·8	48·7	39·9	53	48	47	36	45	SSE.	SW.	calm	S.	...	·01	...	...	38
8.	30·360	30·373	30·304	29·82	30·15	29·90	48·6	49·4	47·9	53	49	47	38	45	S.	SW.	calm	S.	...	...	...	...	46
9.	30·342	30·351	30·327	29·69	29·84	30·11	50·7	51·8	47·9	53	39	53	45	43	SW.	SW.	calm	S.	...	·06	...	...	44
10.	30·476	30·559	30·280	29·92	30·36	30·36	43·8	44·0	43·9	53	28	41·5	34½	41	NW.	NE.	calm	SSW.	·091	...	·05	...	40
11.	30·462	30·485	30·371	29·92	30·24	30·08	41·4	42·0	37·9	49	41	37	43	43	S.	SW.	W.	SSW.	...	...	...	...	40
12.	30·300	30·326	30·261	29·75	30·10	30·18	45·9	48·8	41·2	48	31	43	45	36	S.	SW.	W.	NW.	...	·04	...	...	41
13.	30·488	30·385	30·315	29·97	30·25	29·91	40·7	48·8	38·2	51	38	36	38	41½	SW.	W.	calm	SW.	·061	...	...	...	40
● 14.	29·910	30·083	29·934	29·29	29·50	29·80	48·0	48·7	40·6	53	31	49	45	35	S.	W.	W.	W.	...	...	...	0·24	40
15.	30·104	30·109	29·873	29·54	29·81	29·50	39·3	52·3	36·4	51	40	37	37	42	SW var.	W.	calm	SW.	...	·14	...	...	37
16.	29·520	29·554	29·553	29·08	29·27	29·28	43·3	49·2	39·3	47	32	41	34	33	NW.	W.	calm	SW.	·055	...	·12	...	41
17.	29·494	29·504	29·467	29·05	29·30	29·32	35·8	45·0	34·2	46	26	34	31	31	S.	W.	calm	NW.	...	...	...	...	32
18.	29·464	29·686	29·474	29·15	29·65	29·70	37·3	37·8	34·2	38	23	31·5	33	30	S.	W.	calm	E.	·055	·30	...	...	35
19.	29·590	29·639	29·452	29·25	28·66	28·58	32·8	38·3	30·5	40	30	31	28⅓	34	NW.	SE.	calm	N.	·033	...	...	...	29
☽ 20.	29·374	29·944	29·394	29·20	28·86	30·25	38·3	39·6	32·4	39	34	37	34	28½	NE.	NE.	E.	SW.	...	·14	...	...	33
21.	30·238	30·274	30·214	29·90	30·24	29·82	35·9	39·8	35·0	42	32	36	27½	32	NE.	NE.	E.	SW.	...	·20	...	...	23
22.	29·836	29·875	29·728	29·43	29·60	29·55	42·4	43·3	34·9	51	41	37	38	37	S.	W.	W.	W.	·116	·24	...	...	36
23.	29·544	29·822	29·559	29·09	29·51	29·64	49·8	50·9	43·0	52	31	43·5	37½	36½	S var.	NW.	calm	W.	·283	·03	·51	...	40
24.	29·810	29·828	29·657	29·34	29·70	29·60	38·6	51·3	35·4	51	34	38	36½	32½	W.	W.	calm	W.	...	·04	...	0·28	35
25.	29·672	29·961	29·682	29·21	29·60	29·50	38·8	47·6	36·3	51	29	37	35	34	W.	NW.	NW.	W.	...	·06	·02	...	35
26.	30·028	30·052	29·966	29·49	29·95	29·80	35·2	44·8	33·7	43	26	33	32	39	W.	W.	NW.	W.	·052	...	·09	...	32
27.	29·866	30·000	29·863	29·40	29·64	29·82	41·2	42·4	35·2	51	31	36	38	35	S.	W.	NW.	SW.	...	...	...	...	33
○ 28.	30·176	30·196	30·159	29·70	29·98	29·95	39·3	46·5	35·7	52	40	38	39	42	SW.	W.	calm	SW.	...	...	...	...	32
Mean.	29·977	30·070	29·932	29·53	29·81	29·79	40·5	44·7	37·1	47·64	33·67	38	35·4	36·5					Sum. 1·337	2·19	1·10	1·57	Mean. 35·0

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L. *Researches in the Undulatory Theory of Light, continued; on the Elliptical Polarization produced by Quartz. Part* II. *By* J. TOVEY, *Esq.*

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

IN the former part of this investigation (present volume, p. 172) the terms involving A_3, A'_3, were supposed to be insensible; but the comparison of the results of the calculation with the observed phænomena, by which every supposition ought to be verified, affords no proof of this; for if we restore the neglected terms, by adding respectively, to the expressions (21.), $-A_3 \frac{4\pi^2}{\lambda_{,}^2}$, $-A'_3 \frac{4\pi^2}{\lambda_2^2}$, and (26.) half the same values divided by $A_{,}^{\frac{1}{2}}$, the expression (27.) will still result by neglecting merely the difference of those terms.

We must also observe that there is nothing to determine whether the equations (19.), (20.), give $\rho_{,} = -1$, $\rho_2 = 1$, or $\rho_{,} = 1$, $\rho_2 = -1$. But, since $B_2\, k^3$ is incomparably the largest term in σ', the sign of this quantity must depend on that of B_2; therefore, since the signs of $\rho_{,}$, ρ_2, depend, by (20.), on that of σ', it follows that they also depend on that of B_2. This circumstance shows that a change in the sign of B_2 does not affect the signs of the last terms either in (18.), or in (21.), and (26.), derived from (18.). But, since a change in the signs of $\rho_{,}$, ρ_2, causes a change in those of ζ and tan α, by (22.) and (23.), it follows that, although a change in the sign of B_2 does not affect the velocities $v_{,}$, v_2, it causes a change in the direction of the angle α, through which the

plane of polarization is turned by the passage of the light through the crystal. It is well known that different varieties of quartz turn the plane of polarization in different directions, the crystals being called *right-handed* or *left-handed* accordingly.

We now proceed to investigate the forms assumed, within the crystal, by the surfaces of waves diverging from any given point of it. Suppose the coordinates x, y, z, to originate at the given point, which denote by O; from O conceive a line $O x$, to be drawn parallel to the axis of the crystal, and let the axes of x, y, z, be denoted by $O x$, $O y$, $O z$. Take $O x_{\prime}$, $O x$, $O y$, in the same plane, and suppose the molecules to be so arranged that the system of coordinates may be turned on the axis $O x$, without affecting the values of the sums (16.); then, by art. 4, vol. ix. p. 421, $B_{\prime}$ will vanish, as we have supposed, and the values of $v_{\prime}^2$, v_2^2, will be expressed, approximately, by the equations (18.). Hence we may take

$$\begin{aligned} v_{\prime}^2 &= A_{\prime} - A_3 k_{\prime}^2 - \rho_{\prime} \sin b \,.\, B_2 k_{\prime}, \\ v_2^2 &= A_{\prime}' - A_3' k_2^2 + \frac{\sin b \,.\, B_2 k_2}{\varrho}. \end{aligned} \quad (28.)$$

The equations (12.) and (13.) give

$$\rho_{\prime} = -\frac{s_{\prime}}{\sigma' \cos b - \sigma \sin b}; \quad (29.)$$

$$\rho_2 = \frac{s_{\prime}}{\sigma' \cos b + \sigma \sin b};$$

and therefore, since $s_{\prime} s_{\prime}' = \sigma'^2 \cos^2 b - \sigma^2 \sin^2 b$, (pres. vol. page 169,) we find

$$\rho_{\prime} \rho_2 = -\frac{s_{\prime}}{s'_{\prime}}: \quad (30.)$$

hence, by supposing $s_{\prime}$ nearly equal to $s_{\prime}'$, we have

$$\rho_{\prime} \rho_2 = -1, \text{ nearly}; \quad (31.)$$

which, by (28.) gives

$$v_{\prime}^2 - v_2^2 = A_{\prime} - A_{\prime}', \text{ nearly}. \quad (32.)$$

If we were to leave out of each of the equations (28.) the two last terms, which are comparatively very small, these equations would coincide with (3.) of article 6, vol. ix. p. 422; whence it follows that the investigation comprised in the articles 6—12, of that paper, would apply to the present case, and, consequently, the forms of the surfaces of diverging, or converging, waves are nearly the same in quartz as in an ordinary mineral crystal. This is confirmed by experiment.

It has been shown, in the investigation just referred to, that the waves diverging from the point O will consist of two

sets, of which one set will be spherical and the other spheroidal.

Let $\eta_{\prime}$, $\zeta_{\prime}$, denote the displacements connected with the former, and η_2, ζ_2, those connected with the latter; then $\eta = \eta_{\prime} + \eta_2$, $\zeta = \zeta_{\prime} + \zeta_2$, and the equations (22.) resolve themselves into

$$\left.\begin{aligned}
\eta_{\prime} &= a_{\prime} \sin \left\{\frac{2\pi}{\lambda_{\prime}}(v_{\prime} t - x)\right\}, \\
\zeta_{\prime} &= \rho_{\prime} a_{\prime} \sin \left\{\frac{2\pi}{\lambda_{\prime}}(v_{\prime} t - x) - b\right\}, \\
\eta_2 &= a_2 \sin \left\{\frac{2\pi}{\lambda_2}(v_2 t - x)\right\}, \\
\zeta_2 &= \rho_2 a_2 \sin \left\{\frac{2\pi}{\lambda_2}(v_2 t - x) - b\right\}.
\end{aligned}\right\} \quad (33.)$$

From these expressions it appears, that, in each set of waves, the revolutions of the molecules are elliptical. And, since the equations (31.) show that ρ_2 is nearly the negative reciprocal of $\rho_{\prime}$, it also appears, from these expressions (33.), that the greater axis of the ellipse of revolution in one of the sets of waves has the same direction as the lesser axis of that in the other, and that the revolutions connected with the one set are performed in an opposite direction to those connected with the other. We have seen that, when x coincides with the axis of the crystal, we have $\rho_{\prime} = -1$, $\rho_2 = 1$, hence we perceive that the ellipses are then changed into circles; the motions constituting the two rays being expressed by the equations (23.). All these deductions are verified by the experiments of Mr. Airy.

Having thus, by the aid of certain probable suppositions respecting the relative values of the arbitrary quantities, brought our formulæ to express the laws of the refraction of quartz, we may in conclusion observe, that our general integrals are susceptible of extensive application; and I think I shall be able to show, in another paper, that they are adequate to explain the cause of the absorption of light. It is immediately obvious, from the equations (18.), that, for media possessing, in any degree, the elliptically polarizing structure, the *dispersion-formula* must be different from that which has hitherto been used. I am, Gentlemen, yours, &c.,

Littlemoor, Clitheroe, March 20, 1839. JOHN TOVEY.

P.S. In the last paper, page 169, line next above the equation (12.), *for* b' *read* b; p. 171, *for* A_3 in the second line of (18.) *read* A'_3; and p. 174, line 10, *for* correctly *read* approximately.

LI. *Observations on some of the Products obtained by the Reaction of Nitric Acid on Alcohol. By* Golding Bird, *M.D., F.L.S., G.S., &c. Lecturer on Natural Philosophy at Guy's Hospital.**

FEW subjects connected with organic chemistry have engaged more the attention of chemists than the action of nitric acid on alcohol, from the discovery of hyponitrous æther by Navier, up to the present moment. The composition of what has been long termed sweet spirits of nitre, is now well determined, and no doubt exists of its being a combination of hyponitrous acid with æther ($4\,C\ 5\,H\ 1\,O + N\ 3\,O$). Some of the products formed simultaneously with this æther have been less completely and satisfactorily understood; chemists have however for the most part agreed in stating malic, oxalic, acetic, and carbonic acids to be among the new-formed products, together with a substance mentioned by Thenard as "*très facile à charbonner.*" Taking advantage of the residue left in the retort after the preparation of sp. æth. nitrici on the large scale, I submitted it to examination and obtained some interesting and unexpected results. In the pharmaceutical laboratory of Guy's Hospital, this æther is prepared by distilling in a sand-heat two gallons of alcohol of about ·850, and 24 ounces of nitric acid of 1·50, the operation being stopped when about $3\frac{1}{2}$ pints of fluid are left in the retort. This fluid is straw-coloured, varying in specific gravity from 1·03 to 1·06, strongly acid, and possessing the agreeable odour of hyponitrous æther. When carefully neutralized by a solution of potass it assumed a rich orange-brown tint, and became slightly turbid; the fluid thus saturated precipitated acetate of lead most copiously, but did not affect the limpidity of lime-water or a solution of chloride of calcium, and hence did not contain oxalic acid, although this substance is generally stated to be present in the residue of the distillation. The most remarkable phænomenon, however, occurred when some of this carefully neutralized fluid was exposed to heat in a porcelain bason: as soon as it became warm the colour deepened in tint, in a short time becoming of a rich chocolate brown; a most disagreeable and penetrating odour, something like that of boiling soap, being evolved: in this experiment the temperature of the fluid did not exceed 160° Fahr., the change commencing at 115° Fahr., and went on briskly at 120° Fahr. For the purpose of determining the

* Read before the Chemical Section of the British Association, at Newcastle-on-Tyne, August 1838: and communicated by the Author.

nature of the substance concerned in these phænomena the following experiments were performed.

A. About a pint of the acid yellow fluid remaining after making sp. æth. nit. was submitted to distillation in a glass retort, the temperature applied being just sufficient to keep up gentle ebullition: the first few ounces of fluid that distilled over possessed the flavour and odour of hyponitrous æther, and underwent no change of colour by the addition of a solution of potass; after these had passed over and the receiver been changed, distillation was carried on until orange-coloured fumes appeared in the retort; the fluid collected in the receiver was then removed: its specific gravity was 0·980: it reddened litmus paper, possessed an odour of æther mixed with one so irritating that it produced smarting of the nostrils and eyes with copious lacrymation; its flavour was at first grateful, rapidly however becoming acrid like cayenne pepper, leaving a burning sensation in the mouth and fauces.

B. To this distilled fluid a solution of pure potass, of spec. gr. 1·06, was added; the mixture instantly assumed a fine orange colour, which on the application of a gentle heat deepened in tint, a strong penetrating "soapy" odour being evolved.

C. The excess of acid in the distilled fluid was next neutralized with ammonia, and a solution of nitrate of silver added; a copious white precipitate occurred, which on being warmed over a spirit lamp became dark and converted into reduced silver, giving the fluid a bluish tint when viewed by transmitted light.

D. From these experiments I at first suspected the presence of formic acid, as this (as is well known) readily reduces silver-salts, although the peculiar action of potass is by no means reconcileable with the known properties of that acid. To ascertain whether this acid was really present I neutralized exactly some of the distilled fluid with a weak solution of potass and evaporated it over a vapour-bath to dryness; a nearly black residue, containing a few acicular crystals, resulted. This was placed in a retort mixed with sulphuric acid, and distilled, but not a trace of either formic or acetic acid could be detected in the distilled fluid, which appeared to contain a small quantity of nitric acid. It hence appeared evident, that formic acid could not be the reducing agent in Exp. C, whilst from this circumstance, from the action of potass, and the peculiar odour, I was led to suspect the presence of *aldehyd*.

E. Some of the distilled fluid was neutralized with potass and warmed; when it had assumed the orange-brown tint it

was poured into some very dilute sulphuric acid; a yellow powdery deposit fell; this on the application of heat collected into a resinoid mass, soluble with tolerable readiness in alcohol and æther, resembling in its properties the resin of aldehyd described by Liebig in the second number of his excellent *Handwörterbuch der Chemie*. This circumstance, together with reduction of metallic salts, and the peculiar irritating soap-like odour, left scarcely a doubt of the presence of aldehyd. It now became an interesting question to determine the conditions of the formation of this substance, particularly as the fluid from which this aldehydiferous fluid had been separated by distillation copiously precipitated salts of lime after neutralization by ammonia, indicating the presence of oxalic acid, which as I have already mentioned was absent in the residual fluid previous to this second distillation, and consequently must be considered as a secondary product, resulting from the reaction of nitric acid on some substance formed in the earlier stage of the operation.

F. Some hyponitrous æther was then prepared without heat, by Dr. Black's process, by allowing nitric acid and alcohol to react on each other through a stratum of water in a cylindrical vessel: after the action had continued for two days, some of the lighter part of the fluid was decanted into a retort; it was strongly acid, but smelt agreeably of nitrous æther. On submitting it to distillation a colourless fluid was collected in the receiver; that resulting from the earlier part of the distillation was not at all acrid, nor was it altered by a solution of potass; whilst that which came over last was intensely acrid in flavour, reduced salts of silver after the addition of ammonia, and underwent those changes with weak solutions of potass which characterize aldehydiferous fluids. This experiment was by no means satisfactory as showing the formation of aldehyd by the action of nitric acid on alcohol in the cold, in consequence of the acidity of the fluid previous to distillation; enough nitric acid being probably present in the retort to produce this peculiar substance by the reaction on alcohol or æther on the application of heat. To determine this fact with greater accuracy, some of the acid æther prepared by Dr. Black's process was digested with protoxide of lead and then distilled over some of this oxide: the fluid in the receiver was very mild in flavour, of spec. gr. 0·926, and underwent no change in colour by the addition of alkaline solutions: it was also destitute of action on salts of silver; hence the absence of aldehyd may be safely inferred. From this circumstance we may conclude that this curious substance is by no means a *necessary* result of the action of nitric acid on alcohol

during the formation of hyponitrous æther. The acid fluid left in the cylinder in Dr. Black's process, after these two portions of æther had been decanted, was set aside very loosely covered with a glass plate. In the course of a few days it evolved a pungent odour of acetic acid; it was neutralized with carbonate of soda, and evaporated to a small bulk; numerous crystals of nitre appeared; the supernatant fluid was evaporated to dryness: a crystalline and exceedingly deliquescent mass resulted; this when treated with sulphuric acid evolved copious fumes of acetic acid.

I next turned my attention to the non-volatile products resulting from the action of nitric acid on alcohol. Some of the residual fluid in the retort, after preparing sp. æth. nit. on the large scale, of sp. gr. 1·06, was carefully saturated with carbonate of soda: the neutral fluid after being warmed to get rid of carbonic acid did not trouble solutions of chloride of calcium, and was therefore free from oxalic acid; a solution of acetate of lead was added until no further troubling ensued; the copious precipitate that fell was collected, well washed, and drained on a filter. This substance was diffused through water, and submitted for twelve hours to a current of sulphuretted hydrogen until no more gas was taken up. The mixture was boiled and filtered. The colourless fluid thus obtained was acid, but by very careful evaporation did not evince any tendency to crystallize: it was divided into two portions; one was carefully neutralized by ammonia and the unsaturated portion added. By very careful evaporation over a vapour-bath delicate acicular crystals of an acid ammoniacal salt were obtained. These crystals did not contain malic acid, the presence of which I had expected, for the precipitate their aqueous solution yielded with salts of lead did not dissolve in any appreciable quantity in hot water, nor did it fuse at 212°. As far as I have examined the acid combined with the ammonia in these crystals, I am disposed to regard it as that which has long been confounded with malic acid, viz. oxal-hydric acid, a substance produced by the action of nitric acid on many of the products of organization, often simultaneously with oxalic acid $(4\,C\,3\,H\,6\,O) = \overline{4\,C, 6\,O} + 3\,H$.

A fresh portion of the fluid left after the distillation of nitrous æther was evaporated slowly to a syrupy consistence; nitrous acid fumes appeared in abundance: the whole being allowed to cool deposited in a few hours numerous crystals, which were readily distinguished as those of oxalic acid. The mother liquor was decanted, and submitted to fresh evaporation and set aside: by cooling, delicate foliaceous crystals, possessing the pearly lustre of cholesterine, appeared in the fluid;

these were separated and drained on filtering paper: on being subsequently examined, they also proved to be oxalic acid like the first formed crystals, from which they altogether differed in their physical characters. I am quite unable to account for the curious forms assumed by the second crop of oxalic acid crystals; not having submitted them to analysis I cannot state whether they contained the same quantity of water of crystallization as those possessing the ordinary crystalline form.

Having thus shown that oxalic acid is not a constant product during the preparation of nitrous æther, at least in the earlier stages of the operation, I was anxious to ascertain whether the evolution of aldehyd and formation of oxalic acid bore anything like a mutual relation. For this purpose the following experiment was performed. A quantity of the fluid left in the retort after the preparation of sp. æth. nitrici was placed in a tubulated retort, and a quilled receiver was so adapted as to allow a receiving bottle placed beneath to be changed and removed at will. A sand-heat was then applied and distillation commenced: when about one fourth of the fluid had passed over, the receiving bottle was charged and the fluid examined; it was colourless, aromatic, of sp. gr. 0·861, was unaffected or nearly so by the addition of alkaline solutions, and appeared to be merely an alcoholic solution of hyponitrous æther which had not been completely removed by the operation on the large scale. Some of the fluid in the retort was removed and examined; it was nearly totally free from oxalic acid, and appeared to contain among new products only the oxal-hydric acid. Heat was again applied to the retort, and about a third of the remaining fluid was distilled over; this was of sp. gr. 0·872, exceedingly acrid, like capsicum in flavour, acid, and assumed a deep orange tint by the addition of solution of potass even in the cold; this increased by heat, and the subsequent addition of dilute sulphuric acid produced a copious deposition of the resin of aldehyd. The fluid in the retort contained a large quantity of oxalic acid, formed probably by the reaction of the free nitric acid present upon the oxal-hydric acid formed in the first stage of the operation. Upon a third application of heat distillation was carried on until red fumes appeared; the fluid in the receiving bottle was of sp. gr. 0·972, very acid, scarcely contained any æther, and much less aldehyd than the portion previously distilled, as it was merely tinged yellow by potass: the fluid left in the retort was so loaded with oxalic acid that it crystallized by cooling.

From these experiments, one fact at least of pharmaceutical

importance may be inferred, viz. that in preparing the sp. æth. nitrici for medical uses, the distillation ought to be stopped when oxalic acid appears in the retort, to avoid the presence of aldehyd in the distilled fluid, and consequently that the limitations with regard to the quantity of product obtained, mentioned in the Pharmacopœia, should be strictly attended to, as the presence of so pungent and irritating a fluid as aldehyd in the sp. æth. nit. of the shops, cannot but interfere considerably with the cooling febrifuge uses of the latter. I may remark, that as in the preparation of this æther on the large scale it is of importance to the manufacturer to obtain as large an amount of product as possible, we find, as might be expected, traces of aldehyd in most commercial specimens of the æther, which indeed is indebted to it for that pungent acrid flavour it so frequently possesses. If the directions of the London Pharmacopœia be observed, the product is always free from aldehyd; this I have verified in several instances in numerous specimens prepared in the pharmaceutical laboratory of Guy's Hospital, as well as in that of my friend Mr. R. Phillips, of St. Thomas's Hospital.

In conclusion, I may be permitted to submit the following as the inferences deducible from the foregoing observations:

1. That in the preparation of sp. æth. nitrici, as long as the latter with alcohol only distils over, no oxalic acid is produced, an acid which appears to be identical with the oxalhydric only appearing in the retort.

2. That on continuing the distillation beyond this point, the free nitric acid in the retort reacts on the oxalhydric and produces oxalic acid.

3. That in the action of nitric acid on alcohol in the cold as in Dr. Black's process for the formation of hyponitrous æther, acetic acid is copiously produced, instead of, or in addition to, oxalhydric acid.

4. That aldehyd (as has been before shown by chemists) is produced by the action of nitric acid on alcohol, but that it is not formed in any quantity, or at least does not appear in the distilled fluid, until the formation of hyponitrous æther has nearly or altogether ceased.

5. That the production of aldehyd and oxalic acid are nearly simultaneous, and that both these appear to result from secondary action of nitric acid upon products formed in the early stages of the operation.

6. That the "crystals of Hierne" formed when distillation is carried on until red fumes appear are oxalic acid, notwithstanding their remarkable micaceous form; and that the sub-

stance *très facile à charbonner*, mentioned by Thenard, is probably aldehyd, which from its behaviour towards alkalis might apparently merit that character.

LII. *Answer to the Objections published against a general Theory of the Visual Appearances which arise from the Contemplation of Coloured Objects.* By J. PLATEAU, *Professor at the University of Ghent.*

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN, Ghent, April 15, 1837.†

I HAVE the honour to transmit you herewith an article which I should thank you to insert in your Journal. Although the main object of this paper is not to make known new facts, you are well aware that theoretical discussions are not void of interest with regard to science. Besides, when a theory is attacked, it is naturally to the organs of scientific publicity that the office belongs to furnish the author of that theory with the means of defending his ideas. As it was in England that the principal objections against my statements appeared, and as a part of those objections were published in your Journal; lastly, as my article contains a mere scientific defence, I make no doubt, Gentlemen, that you will willingly devote your pages to it, and do me the favour to insert it in your next Number.

I remain, Gentlemen, yours, &c.,
JH. PLATEAU,
Professor at the University of Ghent.

In publishing my theory on the visual appearances which succeed the contemplation of coloured objects, and on those which manifest themselves during that contemplation ‡, I conceived that if this work deserved any attention, it was principally on account of the generality and simplicity of the point of view under which I have united four different classes of phænomena whose affinity to each other has not been acknowledged; namely, *the duration of impressions on the retina, the*

† This communication, as I subsequently learnt from M. Plateau, was forwarded from Ghent at the date which it bears, and having, after being long given up for lost, at length come to hand through some unknown channel, it is now published without further delay.—R. T.

‡ See *An. de Chim. et de Physique* of Messrs. Gay-Lussac and Arago, August 1833, page 386, and April 1835, page 337. Supplement to the Treatise on Light of Sir J. Herschel, by M. Quetelet, page 515. Memoirs of the Academy of Brussels, tom. viii.

accidental colours by succession, *the irradiation*, and *the accidental colours by simultaneousness*, or *the effects of the juxtaposition of colours*. Above all, I should have thought that if my inquiries had given rise to any refutation, it would have been directed against my general principle, against the law of continuity on which I have made these various phænomena depend: however such has not been the case. It is true, several objections have appeared; but they merely relate to some particular facts, or at most to that class of phænomena which constitute the accidental colours of the first-named species.

Be that as it may, I shall successively examine these different objections; but in order to make them more easily appreciated, it will be necessary first to restate summarily the principles of my theory.

I have divided the appearances in question in two large sections: the first comprehends those which *succeed* the contemplation of objects; that is to say, *the duration of impressions*, and *the accidental colours by succession*, or which show themselves after the disappearing of the objects which produce them. In the second are contained the appearances which *accompany* the contemplation of objects: that is to say, *the irradiation*, and *the accidental colours by simultaneousness*, or which manifest themselves in presence of the objects looked at.

Now, if we consider that the phænomena of the first section are produced *from the moment when the object ceases to act upon the retina, to the one in which the organ recovers its normal state*, and that, on the other hand, those of the second section surround, on the retina, the space directly excited by the light, *from the contour of that space to those parts of the organ which remain in their normal state*, we may say that the first constitute, in the organ, the passage from the state of excitation to the normal state *with regard to time*, and that the others constitute the passage from the state of excitation to the normal state *with regard to space*.

Upon examining the laws which regulate each of these passages, I have remarked a very striking analogy between them; the one being, as it were, but the translation of the other, by substituting space for time. The facts already known, together with my own observations, have induced me to acknowledge that these laws are as follows.

In the first case, that is, in the passage *with regard to time*, the retina being suddenly left to itself after an excitement sufficiently prolonged, retains for some length of time this same state of excitement, which gradually vanishes, to give

place to a state *of nature directly opposite*. The first of these two effects constitutes *the duration of the primitive impression*, and the second the apparition of *the complementary accidental colour*. These are the most manifest effects; but afterwards the impression almost always undergoes oscillations more or less regular: frequently the only effect perceived is the accidental colour which alternately appears and disappears; but in certain circumstances, the accidental colour alternates with recurrences of the primitive colour, so that the impression then *passes alternately into two opposite states*, till it completely vanishes.

In the second case, that is, in the passage *with regard to space*, the state of excitement in the retina extends itself to a small distance round the portion directly excited by the light, and beyond that is manifested a state *of nature directly opposite*, from which results the sensation of the complementary colour. The first of these two phænomena constitutes *irradiation*, and the other the second species of accidental colours, or the effects, so ably analysed by M. Chevreul, *of the juxtaposition of colours*. These two principal phænomena are commonly the only ones manifested; but in favourable circumstances, beyond the space occupied by the complementary colour, *the primitive colour is again to be found*, though much weakened; so that oscillations as to time are here substituted, to a certain degree, by oscillations as to space, if I may be permitted so to express myself.

In order to render the statements of the facts more easy, and to recall the opposition between the primitive and complementary impressions, I have named the first *positives*, and the second *negatives*.

Now the discussion of the phænomena of the first section has induced me to make them depend upon the general principle which follows:

When the retina is submitted to the action of the rays of any colour whatever, it resists that action, and tends to recover its normal state, with a gradually increasing force. Then, if the organ be suddenly withdrawn from the exciting cause, it returns to the normal state by a sort of oscillatory motion, the more intense as the action has been further prolonged, a motion in virtue of which the impression passes first from the positive to the negative state, and then continues generally to oscillate, in a manner more or less regular, by becoming weaker, until it has entirely vanished.

Hence the successive opposite states of the impression would be analogous to the successive positions of a body removed from a stable equilibrium, and which alternately transports

itself from one side to the other of its original position of repose, until its movement be completely annihilated. Besides, as I have said in the publications wherein I have made known my theory, I consider this principle of oscillations as general; that is to say, applicable to any organ whatever, and even to moral phænomena.

With respect to the appearances of the second section, they may be compared to the alternations of the two opposite electricities in an insulated conductor which is submitted to the influence of an electrified body; or likewise to the phænomena which a circular plate presents whose centre is made to vibrate, and in which opposite vibrations are separated by circular lines of repose, &c.

Thus the phænomena of the first section would be the effects of the same law of continuity and inertia that we see manifested in a great many instances, when a body removed from a state of stable equilibrium is afterwards suddenly left to itself; and those of the second section would be equally the effects of an analogous law of continuity, which is frequently manifested when only a portion of a body is maintained, in one way or other, in a state differing from its normal state.

The foregoing is a summary of my theory of visual appearances. With regard to the series of proofs upon which I have grounded it, I can only refer to the papers already quoted in the note. The first article of the *Annales de Chimie et de Physique*, as well as the note inserted in the Supplement to the work of Sir J. Herschel, are nothing more than summary expositions of the *ensemble* of my work; but the memoir contained in the collection of the Brussels Academy, and in the number of April 1835 of the *Annales de Chimie et de Physique*, is a complete development of that part of my inquiries which relates to phænomena of succession, or of the first section. I am now occupied with the second part of this memoir, that is to say, with that which embraces phænomena of simultaneousness, or of the second section.

We shall now pass to the objections. The first which came to my cognizance were advanced in an anonymous article of the Edinburgh Review (number for April 1834, page 160, and following*.) Unluckily, the author had no knowledge of my theory but from the first of the two summary expositions of which I have just spoken, and he was consequently not able to judge of it with thorough competence.

* The reader may consider that my answer has been a long time deferred, but it was only very recently that I knew of the objections in question.

His first attack is directed against one of the proofs by which I intend to establish an opposition of nature between accidental and direct impressions. My proposition was thus expressed *:—

" In the cases where the combination of real colours produces *white*, the combination of accidental colours produces *the contrary to white*, or *black*. Whereas, for instance, two real complementary colours produce together *white*, two accidental complementary colours produce together *black*.

" This fact may be ascertained by an experiment: place on a black ground a rectangle of paper, the two halves of which are painted with two complementary colours, for instance red and green, as indicated by the fig. 1. †, the middle of each coloured portions being marked with a black point. Then, if you alternately direct your sight from one of these points to the other, for a sufficient length of time, the result will be an image, in the bottom of the eye, formed by the superposition of the accidental green produced by the red half, and of the accidental red produced by the green half; or, in other terms, by the superposition of two complementary accidental colours. Now, if you suddenly and completely cover your eyes with a handkerchief, this image will appear perfectly *black*, having on the right hand a red image, and on the left a green image (supposing that the green and red halves of the rectangle be placed as in the figure). The production of these two lateral images sufficiently explains itself."

I shall here subjoin some particulars for persons who may wish to repeat the experiment. The rectangle I employed was 20 centimetres long by 10 high, and the ground on which I placed it was a large black shawl spread on the floor, in a light room. I stood with my back to the windows, but so as not to throw a shadow on the object. I then directed my sight alternately on the two black points, looking at each of them for nearly a second, and I continued thus about the space of a minute, after which I covered my eyes, as I before said.

These are now the objections. The anonymous author begins by saying that this property of accidental colours advanced by me, must appear, at first sight, an important one, but that a slight examination of the subject will show, " that this proposition is a verbal illusion, and that the physical fact which it so erroneously expresses, has been long known to philosophers."

* *Ann. de Chimie et de Physique*, August 1833, p. 388.

† In the figure which accompanies the article, (in the *Ann. de Chim.*) the green half of the rectangle is on the right, and the red half on the left.

"An accidental colour," says he in continuation, "is something essentially distinct from a colour produced by the action of direct rays. The rays which produce ordinary colours, can be combined in any proportion we please; and the resulting effect is the sum of the actions of each separate ray upon the retina. Hence all the different colours of the spectrum produce a purely white beam of light; and perfect whiteness may be also produced by two compound colours, one of which is complementary to the other. An accidental colour, however, cannot be added to, or combined with another. When the eye sees an accidental colour, suppose *red*, the excited part of the retina is insensible to all other rays but those of the accidental colour. If we instantly excite the same portion of the retina with another light, which is an accidental *green*, and thus render it insensible to *red*, then the eye will see blackness, not *because the accidental red and the accidental green compose blackness*, but because the eye has been in succession rendered insensible to the two colours which compose white light itself."

Thus the author founds himself upon the hypothesis which attributes accidental colours to a diminution in the sensibility of the retina, as if that were a demonstrated truth. But I have shown at the beginning of the article against which the objections are directed, that this hypothesis is insufficient to explain the phænomena. I have stated in proof of it this fact: "that accidental colours are perfectly seen in the most complete obscurity." In fact, in the hypothesis adopted by the anonymous author, the accidental colour, for instance, red, which succeeds the prolonged contemplation of a green object, is explained by saying that the retina fatigued by this continued excitation of green light, loses its sensibility to this light, and is then only affected by the rays which form the complementary red tint. This ingenious theory perfectly explains the phænomenon, when, wishing to see the accidental colour, we cast our eye on a white surface, as is usually practised. For then we can admit that the red rays, which enter into the composition of whiteness, alone act on the retina. But what does that explanation amount to, when applied to the accidental colour seen in a complete obscurity, and when, in consequence, no ray reaches the eye that can give the sensation of the complementary tint? Now it is by placing my eyes in that condition, as above indicated, that I have observed the result of the combination of two accidental colours complementary to each other; and it is precisely in order to render impossible the explanation of the fact by the diminution

of sensibility of the retina in regard to certain rays, that I have thus made the experiment.

But the anonymous author alleges that an accidental colour cannot be added to, or combined with another. To which I answer by referring him to paragraph 24 of the dissertation of Father Scherffer*, one among the philosophers who have made the greatest researches on the subject of accidental colours, the one precisely who has furnished us with the theory which attributes the phænomena to a partial diminution in the sensibility of the organ. In this paragraph, Scherffer relates direct experiments whereby he convinced himself that the accidental colours combine perfectly well together. If the anonymous author maintains that these experiments can be explained by the diminished sensibility, seeing that Scherffer observed the results of the combination by casting his eye on a white wall, I shall answer that the effects are the same in a complete obscurity, as may be easily ascertained, by substituting for the red and green rectangle of my experiment an orange and green one. Then, if the same process is used, three coloured images will be seen, whose intermediate one will be *violet*. This image is the result of the combination of the blue and red accidental colours.

Consequently, accidental colours are really susceptible of combinations, and the resulting tints are the same as in respect to real colours, except the case in which the two composing accidental ones are complementary to each other. In this latter circumstance, since the result is blackness, it appears to me that I have not *expressed the fact erroneously*, by saying that two complementary accidental colours produce together black, which signifies, if you will, that they mutually destroy each other.

It is difficult for me to conceive how the anonymous author has not paid attention to this circumstance formally expressed in my article, that it is in a complete obscurity I have observed the phænomena. For this is a chief point in my theory of accidental colours, and totally excludes all the explanations which may be founded upon a diminution of sensibility to certain rays. The author thus continues:—

"If Buffon, or Dr. Darwin, or Count Rumford had been asked what would be the effect of exciting the retina in quick succession with all the simple colours of the spectrum, or with two compound colours which compose white light, they would all have immediately answered, *blackness*. M. Plateau

* Journal of Physics, by Rozier, tom. xxvi. year 1785, p. 282.

has therefore, in this part of the inquiry, merely expressed what has been long known, in language physically incorrect, and calculated to convey very erroneous notions on the subject."

Indeed, the philosophers the author speaks of, might have made the answer he attributes to them (except however Buffon, for the theory of the relative insensibility did not appear till a long time after), because they would have supposed that the effect was observed by casting the eye on a white surface. But if they had been asked what should be the effect in a total obscurity, they would doubtless have said that the decision depended on experience. I have therefore not *merely expressed what has been long known.*

We now come to the other objections.

"Plateau maintains," says the anonymous author, "that after the direct or positive impression of the primitive colour has continued visible for a certain time, and gradually faded away, it is succeeded by the negative impression, or accidental colour. But what is original in his theory he maintains, that after the accidental colour has faded away in its turn, *it is succeeded by the primitive colour*, this alternation going on till the impression wears away. If we look into the volume already quoted *, we shall see that the only *novelty* in Plateau's theory is the recurrence of the primitive impression, and the continued alternation of the two; but we do not think that this recurrence and alternation are established by sufficient evidence; at least, we cannot by any contrivance render it visible. It is certain that the accidental colour disappears and returns, and undergoes other changes; but these changes, we conceive, are not the effect of the primitive impression, or a continuation of a necessary series of changes, of which the primitive impression is the commencement; but the result of subsequent actions upon the retina, which M. Plateau has not been careful enough to detect and analyse. It has been proved, for example, that a pressure upon the retina with the finger changes the accidental colour, and it is asserted by Sir Charles Bell, that if we squint or distort the eye, a vivid impression on the retina instantly disappears, as if it were wiped out. When M. Plateau, therefore, saw the accidental colour change into the primitive, was he sure that there was no pressure made upon the retina by the motion of the eye, or by the involuntary closing even of the eyelids, which is sufficient to produce the observed change? That the changes of colour

* This work is the Treatise on Optics of Sir David Brewster, forming a part of the Cabinet Cyclopædia, and in which this philosopher has also presented a theory of accidental colours.

in question are not regular, and are produced by some irregular influence, may be inferred from M. Plateau's own observation, *that the alternations of colour do not always take place in the same manner, that they vary with the sensibility of the eyes, and particularly with the circumstances under which the experiment is made*; and he afterwards remarks, that the regular alternation of the primitive and accidental colour *is the effect most frequently observed.* Now this additional frequency of one phænomenon in a series is no proof of a regular law; and when we consider how the retina is affected by the state of the stomach, by the pressure of the blood-vessels, which may, in some cases, be an intermitting or an alternating one, we must demand a series of distinct experiments made with the same result, on the eyes of different observers accustomed to the examination of this class of phænomena, and aware of the causes which exercise a disturbing influence, before we can admit the conclusion drawn by M. Plateau."

The anonymous writer is strangely mistaken, if he supposes that I attach any value to the *novelty* of my experiments. In support of my theory, I should have considered it more advantageous, had I only to relate facts before ascertained. For, with respect to pure phænomena of sensations, more than in any other circumstance, it may be supposed that the author of a theory is influenced in his judgements, in spite of himself, by the desire of reconciling the facts with his system. Accordingly, in my detailed memoir, I have endeavoured, as much as possible, to establish my statements on anterior observations; and, when this resource failed me, I was most careful in requesting other persons to repeat my experiments. For instance, with respect to the recurrence of the primitive impression, a recurrence that the anonymous author was not able to observe, I have quoted, in my memoir, the following experiment related by Rozier, the editor of the Journal of Physics*:

"Suppose," says he, "any apartment, either deprived of the sun's light, or at least in the moment when it can be said that it is neither day or night (the experiment succeeds better in the first case). Suppose in this apartment a candlestick furnished with a lighted wax taper; the light of a candle or lamp producing the same effect. Place this candlestick at your feet, on the floor; look at this light perpendicularly, so that your eyes remain fixed on it without interruption for several moments. Immediately after, place an extinguisher on this light, lift up your eyes to the wall of the apartment, fix

* See this Journal, tom. vi. page 486, year 1775.

your sight towards the same point, without winking the eye; you will see nothing but darkness at the beginning of this operation, after that, towards the point you look at, *there will appear a much greater obscurity than that of the remainder of the apartment.* Continue thus to keep your sight steadily fixed; gradually in the middle of this obscurity, a reddish colour will manifest itself; *it will become insensibly enlivened, its brightness will increase, and at last it will acquire the colour of the flame.*"

Thus, in this experiment, the obscure negative image, that is to say, contrary to the brilliant impression of the flame, was gradually changed into a new luminous image having the colour of this flame; that is, into a positive image. I must here remark, that Rozier recommends *to fix the sight towards the same point without winking the eye,* and further, *to keep the sight steadily fixed,* so that these precautions remove every supposition of pressure or distortion of the eyes, which could alone, as the anonymous author maintains, cause the image to vary. I shall add, that this experiment has not been made with the view of giving weight to any theory; Rozier presents it simply as a fact that he does not seek to explain. With respect to more multiplied alternations of the two impressions, alternations which the anonymous author has not been more successful in verifying, unluckily no one, as far as I know, had noticed them before me. For which reason I have taken care to associate other persons with myself, who have experienced the same effects, and I have cited among them M. Quetelet, a name which ought to inspire the fullest confidence (see my article of *Annales*, page 392). I will here relate the experiment such as I describe it in that article, pages 393 and 394.

"I applied to one of my eyes a black tube about 50 centimetres long by 3 in diameter, at the same time covering the other eye completely with a handkerchief, and I looked steadfastly, during a minute at least, on a red paper exposed to a clear day-light; after which, withdrawing the tube suddenly without uncovering the other eye, I looked at the white ceiling of the apartment. I then saw a green circular image, which, after some time, gave place to a red one of a feeble intensity indeed, and of a very short duration, but perfectly visible; afterwards, the green colour reappeared, which, soon after, was again succeeded by a reddish image, and thus three or four times successively, the two opposite impressions being less and less intense."

I shall only subjoin that the coloured paper ought to be sufficiently large for its edges not to be perceived. In this

experiment I have seen as many as nine oscillations of the impression produced, that is to say, five transitions from the positive to the negative, and four from the negative to the positive. With regard to disturbing causes which might, according to the anonymous author, have occasioned these modifications in the impression, I must, if such had presented themselves, have been a very unskilful observer, not to have noticed their influence, nor have secured myself against them. I must moreover say that with respect to myself the accidental colours are not so fugitive, and that they by no means disappear from my eyes by distorting them, squinting, or closing the eyelids.

The anonymous author is again mistaken, in attributing to me this assertion, *that the regular alternation of the primitive and accidental colour is the effect most frequently observed.* I have, on the contrary, said (page 392 in the article of the *Annales*), that the effect most frequently observed was that of the disappearances and reappearances of the negative or accidental impression alone. And indeed, among the philosophers who have made researches on accidental colours, many have taken notice of this fact, as, for instance, may be seen in the memoirs of Scherffer and Darwin.

The reader may see from what precedes, that the facts I have advanced are not deficient in authority, as the anonymous author seems to allege; he has there limited his objections; let us, therefore, proceed to those which have been raised by other persons.

[To be continued.]

LIII. *On the Constitution of the Resins.* By James F. W. Johnston, Esq. *F.R.S., Professor of Chemistry and Mineralogy in the University of Durham.*

To Richard Phillips, Esq.

My dear Phillips. Durham, March 20, 1839.

THE more I study the constitution of the resins the more interesting does it become; and I begin now to consider them as presenting one of the most beautiful natural families to be met with in the entire range of organic chemistry. In my last letter I gave you an outline of some of the results at which I had then arrived. You will be interested probably in learning the further development I have already been able to give them. This will best appear by presenting to you an outline of the classification of the resins, so far as they have yet been analysed.

General Irrational Formula $= C_{40}\,H_{32 \pm x}\,O\,y$

$$\text{Group I.} = C_{40}\,H_{32-x}\,O\,y$$
$$\text{II.} = C_{40}\,H_{24-x}\,O\,y$$

In these groups x can never exceed 8, and there may be other groups, none of the members of which are yet known, of which H_{8-x} H_{16-x}, H_{40-x}, may form the middle term.

Each of these groups is divisible into many subgroups, in each of which y is represented by a different number. Thus

Group I. $= C_{40}\,H_{32-x}\,O\,y$

Subgroups.	To which belong		
$\underline{A} = C_{40}\,H_{32-x}\,O_1$	Anime resin	$= C_{40}\,H_{32}\,O_1$	Laurent.
	Fossil copal	$= C_{40}\,H_{32}\,O_1$	Johnston.
$\underline{B} = C_{40}\,H_{32-x}\,O_2$	Elemi resin	$= C_{40}\,H_{32}\,O_2$	Rose.
	Mastic B	$= C_{40}\,H_{31}\,O_2$	Johnston.
$\underline{C} = C_{40}\,H_{32-x}\,O_3$	Anime B, Copal B	$= C_{40}\,H_{31}\,O_3$	do.
$D = C_{40}\,H_{32-x}\,O_4$	Colophony	$= C_{40}\,H_{30}\,O_4$	Liebig.
	Mastic A	$= C_{40}\,H_{31}\,O_4$	Johnston.
$E = C_{40}\,H_{32-x}\,O_5$	Modified colophony	$= C_{40}\,H_{27}\,O_5$	do.
	Modified mastic A, Juniper A	$= C_{40}\,H_{31}\,O_5$	do.
$F = C_{40}\,H_{32-x}\,O_6$	Resin of Guyaquil	$= C_{40}\,H_{26}\,O_6$	do.
	Asphaltene ?	$= C_{40}\,H_{32}\,O_6$	Boussingault.
$G = C_{40}\,H_{32-x}\,O_7$	no examples yet known.		
$H = C_{40}\,H_{32-x}\,O_8$	Pasto varnish	$= C_{40}\,H_{32}\,O_8$	do.
	Berengela resin	$= C_{40}\,H_{31}\,O_8$	Johnston.

Group II. $= C_{40}\,H_{24-x}\,O\,y$

Subgroups.	To which belong	
$A = C_{40}\,H_{24-x}\,O_8$	Resin of dragon's blood	$= C_{40}\,H_{21}\,O_8$
	Resin of gamboge	$= C_{40}\,H_{23}\,O_8$
$B = C_{40}\,H_{24-x}\,O_{10}$	Resin of guiacum	$= C_{40}\,H_{23}\,O_{10}$
$C = C_{40}\,H_{24-x}\,O_{12}$	Acaroid resin	$= C_{40}\,H_{20}\,O_{12}$

This second group I have only just established, and other subgroups will present themselves in the progress of the investigation. You will observe that the subgroups comprehend divisions in which the equivalents of hydrogen vary so that the number of substances which may belong to this natural family is almost endless. The details of the analyses

from which the above, and other results are deduced, will be laid before the Royal Society.

The salts of the resins have also occupied much of my attention, but I have not yet been able to satisfy myself as to their constitution. So far is clear, that in combining with bases the resins do not give off the elements of water. There is a difference I suspect in the number of equivalents of hydrogen present in the resin when combined and uncombined, but the exact nature of that difference requires further elucidation.

Though you do not favour symbolic expressions in chemistry very much, I hope you will allow that there is considerable beauty in the hasty generalizations I have here given you. Believe me, my dear Phillips,

Yours very truly,
JAMES F. W. JOHNSTON.

LIV. *The Bakerian Lecture.—On the Theory of the Astronomical Refractions. By* JAMES IVORY, *K.H., M.A., F.R.S. L. & E., Instit. Reg. Sc. Paris, Corresp. et Reg. Sc. Gottin. Corresp.*

[Continued from p. 287.]

3. IT appears that Newton himself was the first to apply this new method to the problem of the astronomical refractions. A table, which he had computed, and which he gave to Dr. Halley, is published in the Philosophical Transactions for 1721. Nothing is said as to the manner in which the table was constructed; and it has always been a curious and interesting question among astronomers, whether it is the result of theory, or is deduced from observations alone without the aid of theory. Some original letters of Newton to Flamsteed, published in 1835 at the expense of the Board of Admiralty, have put an end to all doubts on this point. These letters prove that Newton studied profoundly the problem of the refractions; that for several months in succession he was occupied almost entirely in repeated attempts to overcome the difficulties that occurred in this research; during which time he calculated several tables with great labour, namely, the one he gave to Halley, and another, or rather three others, which have been found in his letters to Flamsteed lately printed.

Admitting that the refractive power of the air is proportional to its density, which Newton had established in his Optics on speculative principles, and which Hawksbee afterwards was the first to demonstrate experimentally, the mathe-

matical solution of the problem requires a knowledge of the law according to which the densities vary in the atmosphere. In his first attempt Newton assumes that the densities decrease in ascending in the same proportion that the distances from the earth's centre increase. Now a and r denoting the sam e thingsas before, put l for the total height of the atmosphere; then $\phi\ (\rho)$ the refractive power of the air at the distance r from the centre of the earth, will, according to this hypothesis, be expressed by the formula

$$\phi\ (\rho) = \phi\,(\varrho') \times \frac{l-r+a}{l},$$

If this value be substituted in the formula (1.), which is a deduction from the sixth proposition of the first book of the Principia, the result will be

$$d\,.\,\delta\,\theta = \frac{\phi\ (\rho')}{l}\ .\ \frac{d\,\tau^2}{d\,z^2}\,.\,r\,d\,n.$$

In this expression we have

$$\frac{d\tau^2}{d z^2} = \frac{1}{v^2} = \frac{1}{1+2\,\phi\ (\rho)}:$$

and as $2\,\phi\ (\rho)$, or the increment of the square of the velocity of the light is very minute, amounting to less than ·0006 in passing through the whole atmosphere to the earth's surface, we may reckon $\frac{d\,\tau^2}{d\,z^2}$ as unit; thus we get

$$d\,.\,\delta\,\theta = \frac{\phi\ (\rho')}{l}\,.\,r\,d\,n\,;$$

and by integrating

$$\delta\,\theta = \frac{2\,\phi\ (\varrho')}{\left(\frac{l}{a}\right)}.\int \frac{r\,a\,d\,n}{2\,a^2}.$$

This result, which M. Biot has also obtained, is equivalent to the geometrical construction communicated by Newton to Flamsteed in a letter from Cambridge, December 20, 1694. The problem was now reduced to the quadrature of a curve, for which a general method is given in the fifth lemma of the third book of the Principia, a method which is still used when the direct process of integration fails, or becomes too intricate for practice. What has been said not only proves the exactness of Newton's solution of the problem; it also points out, with little uncertainty, the manner in which he obtained it. Of the arithmetical operations of the quadrature there is no account; and they would be of no interest had they been

preserved. He complains much of the great labour of the numerical calculations; but all difficulties were overcome, as was to be expected: a table was computed and communicated to Flamsteed in a triple form, for summer, winter, and the intermediate seasons of spring and autumn. On mature reflection there occurred to him a serious objection to the supposed scale of densities, on which account he writes to Flamsteed that he does not intend to publish the tables. The fault lies in this, that the centripetal force which continually inflects the light to the earth's centre, is the same at all the points of the trajectory, or, in the words of Newton, the refractive power of the atmosphere is as great at the top as at the bottom,—than which nothing certainly can be more different from what actually takes place in nature.

Dismissing his first hypothesis, Newton next turned his attention to the 22nd proposition of the third book of his Principia. If the atmosphere consists of an elastic fluid gravitating to the earth's centre in the inverse proportion of the square of the distance, and if it be admitted that the densities are proportional to the pressures, Newton, in the proposition cited, proves in effect that the densities will form a decreasing geometrical series, when the altitudes are taken in arithmetical progression*. He writes to Flamsteed that an atmosphere so constituted is *certainly the truth.* Newton evidently intended by this assertion to mark a distinction between pressure, which is a cause of the variation of density that actually exists in nature, and his first assumed law of the densities, which is entirely arbitrary. Setting aside hypothesis, he now advanced so far in the true path of investigation; and if the manner in which heat is diffused in the atmosphere and the consequent decrease of density were not known when he wrote, he advanced as far as the existing state of knowledge enabled him to do. It is certain from his letters, that, after much time and labour, he at last succeeded in calculating a table of refractions on the principle that the density is proportional to the pressure. Such a table he communicated to Flamsteed, although it is not found in the letters lately published; and there is every reason to think it the same which he gave to Halley, and which that astronomer inserted in the

* Newton demonstrates strictly that the densities will be in geometrical proportion when the distances from the earth's centre are in musical or harmonical proportion, that is, when they are the reciprocals of an arithmetical progression; but in a series of this kind, if the first term bear an almost infinitely great proportion to the differences of the following terms, as is the case of the radius of the earth when compared to elevations within the limits of the atmosphere, the differences of the terms or the elevations may, without sensible error, be reckoned in arithmetical progression.

Philosophical Transactions for 1721. Two elements only are sufficient for computing all the numbers in a table of refractions constructed by assuming that the density is proportional to the pressure, namely, the refraction at 45° of altitude, and the height of the homogeneous atmosphere, which is deducible from the horizontal refraction. The table of Halley, therefore, contains in itself all that is required for ascertaining whether it was calculated or not by the principle alluded to in the letters of Newton to Flamsteed. Kramp seems to be the first who sought in the table for the manner of its construction; and his discoveries in this branch of science enabled him to assign the height of the homogeneous atmosphere, which is one essential element. The refraction at 45° of altitude, which is the other element, is found in the table equal to 54″, or, in parts of the radius, to ·0002618: and Kramp found 4377* toises for l, the height of the homogeneous atmosphere; so that, if a be the radius of the earth in toises, we have

$$\alpha = \cdot 0002618,$$

$$i = \frac{l}{a} = \cdot 0013356;$$

and the two elements, α and i, are sufficient for computing the whole table, if it be such as is mentioned in the correspondence between Newton and Flamsteed. The formula for the refraction in the supposed constitution of the atmosphere has been given both by Kramp and Laplace; and it may be taken from the paper in the Philosophical Transactions for 1823, p. 441,

$$\lambda = \frac{\alpha}{i} = \cdot 19601, \Delta = \sqrt{\cos^2\theta + 2is},$$

$$\begin{aligned}\delta\theta = \alpha\sin\theta \times \Big\{ & \left(1 - \lambda + \frac{\lambda^2}{2} - \frac{\lambda^2}{6}\right) \cdot \int \frac{ds\, c^{-s}}{\Delta} \\ & + (\lambda - 2\lambda^2 + 2\lambda^3) \cdot \int \frac{2\, ds\, c^{-2s}}{\Delta} \\ & + \left(\frac{3}{2}\lambda^2 - \frac{9}{2}\lambda^3\right) \cdot \int \frac{3\, ds\, c^{-3s}}{\Delta} \\ & + \frac{8\lambda^3}{3} \cdot \int \frac{4\, ds\, c^{-4s}}{\Delta},\end{aligned}$$

the integrations extending from $s = 0$ to $s = \infty$. The coefficients of this formula are as follows:

* *Anal. des Réfractions Astronomiques,* p. 19.

$$A = 1 - \lambda + \frac{\lambda^2}{2} - \frac{\lambda^3}{6} = \cdot 82193$$

$$B = \lambda - 2\lambda^2 + 2\lambda^3 = \cdot 13423$$

$$C = \frac{3}{2}\lambda^2 - \frac{9}{2}\lambda^3 = \cdot 02377$$

$$D = \frac{8}{3}\lambda^3 \doteq \cdot 02007$$

For the horizontal refraction, when $\cos\theta = 0$, $\Delta = \sqrt{2is}$, we obtain by the usual integrations,

$$\delta\theta = \frac{\alpha\sqrt{\varpi}}{\sqrt{2i}} \times \left\{A + B\sqrt{2} + C\sqrt{3} + D\sqrt{4}\right\}:$$

or in seconds, $\delta\theta = 2024{\cdot}2$ instead of $2025''$ as in Halley's table. This proves the exactness of Kramp's elements.

With respect to the other numbers in the table a distinction must be made. In every table of refractions, whatever be the constitution of the atmosphere on which it is founded, the numbers answering to altitudes greater than 16°, depend only upon one element, namely, the refractive power of the air. Reckoning from the zenith as far as 74°, any table may be deduced from any other, provided both are accurately calculated, merely by a proportion. In the table published annually in the *Con. des Temps*, the refraction at 45° is $58''{\cdot}2$: and, if Halley's table has been accurately computed, the numbers in it, between the limits mentioned, will be equal to the like numbers in the French table multiplied by $\frac{540}{582} = \frac{90}{97}$. The calculation being made, the results will be found to agree almost exactly with the short table inserted by M. Biot in the additions to the *Con. des Temps* for 1839, p. 105, the greatest difference between the computed quantities and the numbers in Halley's table, being about $2''$.

But this gives no intimation with respect to the particular constitution of the atmosphere assumed in the calculation of the table. What is peculiar to a table in this respect has no sensible influence on the refractions it contains except at altitudes less than 16°. No definitive opinion can therefore be formed on the question, whether Halley's table is the same which Newton computed and communicated to Flamsteed on the principle that the densities are proportional to the pressures, without comparing it with a sufficient number of re-

fractions at low altitudes calculated from the elements of Kramp. As the settling of this point may be thought not unimportant, the following formula, which affords the means of computing the refractions at all altitudes with exactness, has been investigated by reducing the integrals in the expression of $\delta\theta$ to serieses.

$$\text{Tan } \phi = \frac{2\sqrt{5i}}{\cos\theta}, \quad e = \tan\frac{\phi}{2}.$$

$$\text{Log tan } \phi = \text{log secant } \theta + 19{\cdot}2133569 - 20.$$

$$\delta\theta = \sin\theta \times \left\{ \begin{array}{lrl} & {}'' & \text{log.} \\ e & \times\ 660{\cdot}795 & \ldots\ldots\ 2{\cdot}8200669 \\ +\,e^3 & \times\ 551{\cdot}634 & \ldots\ldots\ 2{\cdot}7337059 \\ +\,e^5 & \times\ 371{\cdot}268 & \ldots\ldots\ 2{\cdot}5696873 \\ +\,e^7 & \times\ 219{\cdot}762 & \ldots\ldots\ 2{\cdot}3419630 \\ +\,e^9 & \times\ 116{\cdot}763 & \ldots\ldots\ 2{\cdot}0673034 \\ +\,e^{11} & \times\ 58{\cdot}170 & \ldots\ldots\ 1{\cdot}7646976 \\ +\,e^{13} & \times\ 28{\cdot}275 & \ldots\ldots\ 1{\cdot}4514092 \\ +\,e^{15} & \times\ 13{\cdot}797 & \ldots\ldots\ 1{\cdot}1397974 \\ +\,e^{17} & \times\ 6{\cdot}806 & \ldots\ldots\ 0{\cdot}8329041 \\ +\,e^{19} & \times\ 3{\cdot}311 & \ldots\ldots\ 0{\cdot}5199046 \end{array} \right\}$$

The series converges very slowly, which has made it necessary to continue it to ten terms, the amount of which is still $3''{\cdot}6$ deficient from the exact quantity $2024''{\cdot}2$. As the last terms decrease in the proportion of 2 to 1, it is obvious that the true sum would be obtained by continuing the series: but the terms set down are more than sufficient for the present purpose.

The exact refractions calculated by the formula are next to be compared with the numbers in Halley's table.

Apparent zenith. dist.	Refractions		Difference.
	Computed.	Halley's Table.	
°	′ ″	′ ″	″
20	19·6	20	—0·4
40	45·2	45	+0·2
60	1 33·1	1 32	+1·1
70	2 27·0	2 26	+1·0
80	4 55·2	4 52	+3·2
82	6 3·9	6 00	+3·9
84	7 45·0	7 45	0·0
86	10 53·1	10 48	+5·1
87	13 22·5	13 20	+2·5
88	17 4·6	17 8	—3·4
89	23 3·5	23 7	—3·5

The examination that has now been made fully establishes

that Halley's table is no other than the one which Newton computed on the supposition that the densities in the atmosphere are proportional to the pressures. The discrepancies, amounting in every instance to a small part of the whole quantity, are such as might be expected to occur unavoidably in the intricate and laborious methods of calculation followed by Newton. Some part of the differences may also arise from the elements of the formula, which, being deduced from only two numbers of the table, may not exactly coincide with the fundamental quantities which Newton assumed. M. Biot, by a method different from Kramp's, has found other elements, which make the horizontal refraction $8''\cdot3$ less than in the table; but these small variations, which proceed from calculating in different ways, only confirm more strongly that Halley's table is the same which Newton constructed by his second hypothesis, and communicated to Flamsteed.

It appears from what has been said, that, as far as the mathematics is concerned, the problem of the astronomical refractions was fully mastered by Newton. It would be strange indeed to suppose that the author of the theories in the Principia would find difficulty to apply them in a case to which he bent the whole force of his mind. But he was embarrassed by the suppositions he found it necessary to make respecting the physical constitution of the atmosphere. His first hypothesis is evidently contrary to nature, in admitting that air at all altitudes exerts the same power to inflect the light to the earth's centre; his second method made the refractions too great near the horizon, thereby proving the necessity of searching out some new cause for the purpose of reconciling the theory with observation. Averse from hypotheses, he seems, on these accounts, to have declined inserting in his works a problem which had cost him so much labour, and upon his solution of which he evidently set some value.

If the different attempts to solve this problem are to be tried with the same rigour that Newton judged his own, it must be decided that they are all liable to objections. They all involve some supposition that has no foundation in nature; or they leave out some necessary condition of the problem. It is allowed that the variation of heat at different altitudes is unknown; that we are equally unacquainted with the manner in which is diffused the aqueous vapour that is always found, more or less, in the atmosphere; that the law of the densities has not been ascertained. But besides these capital points, the accurate M. Poisson will suggest other properties that must be attended to in an atmosphere in equilibrium: the conducting power of heat varying with the condition of the

air; its power of absorbing heat; and the interchange by radiation which takes place with the earth, with the etherial spaces above, and with the stars visible above the horizon. So many conditions, placed beyond the reach of our inquiry, may well puzzle the most expert algebraist to take them into account. But it may be doubted whether this be the proper view of the problem. The astronomical refractions at any observatory are mean effects of the atmosphere; and it may be alleged that the proper way of accounting for them is to compare them with other mean effects of the atmosphere at the same place.

4. In 1715, twenty years after the researches of Newton, Brook Taylor published in his *Methodus Incrementorum*, the first investigation of the astronomical refractions on the supposition that the density of the air is variable. The differential equation is accurately deduced from the principles laid down by Newton; which removed all the difficulties of the problem in this respect.

Kramp, in his *Analyse des Réfractions Astronomiques et Terrestres*, has elucidated the elementary parts of the problem, and has greatly improved the mathematical solution. His method is particularly commodious and useful in the case of the horizontal refraction. For altitudes above the horizon the integrals are not susceptible of being simply expressed, and seem to require the aid of subsidiary tables in applying them.

The problem of the refractions being an important one in astronomy, many solutions of it have been published by different geometers. Some of these are preferable to others, because the method of calculation is easier in practice. For altitudes greater than 16°, they may all be reckoned equivalent, differing from one another only in the quantity assumed for the refractive power of air. They also mostly agree in the horizontal refraction, which is taken from observation. But for altitudes less than 16°, they are different; because, at these low altitudes, the refractions are affected by the arbitrary suppositions used in constructing the tables.

Thomas Simpson published a judicious dissertation on this problem. He distinctly points out that, to a considerable distance from the zenith, the refractions are independent of the manner in which the atmosphere is supposed to be constituted. In comparing the two atmospheres that have densities decreasing in arithmetical and geometrical progression, he remarks that the horizontal refraction comes much nearer the observed quantity in the first atmosphere than it does in the second; for which reason he gives the preference to the first

as likely to represent the phænomena with greater accuracy. Now in this his reasoning is not much different from the argument afterwards used by Laplace, to prove that the same two atmospheres are limits between which the true atmosphere is contained.

Newton likewise found that the refractions computed according to his second method, that is, in an atmosphere with densities decreasing in geometrical progression, are too great near the horizon, on which point he thus writes to Flamsteed: "Supposing the atmosphere to be constituted in the manner described in the 22nd proposition of my second book (which certainly is the truth), I have found, that if the horizontal refraction be 34′, the refraction at the altitude of 3° will be 13′ 3″; and if the refraction in the apparent altitude of 3° be 14′, the horizontal refraction will be something more than 37′. So that instead of increasing the horizontal refraction by vapours, we must find some other cause to decrease it. And I cannot think of any other cause besides the rarefaction of the lower region by heat." Here the true reason is assigned why the refractions near the horizon, in an atmosphere constituted as supposed, so much exceed the observed quantities. When the density is made to depend solely on the incumbent weight, the air is not rarefied enough; and the greater density causes a greater refraction. Having correctly estimated the effect produced by the pressure of the supported air, Newton is unavoidably led to ascribe to heat the greater rarefaction that takes place in the atmosphere of nature. His words prove that he had no clear conception in what manner the density in the lower region is altered by the agency of heat; and, to say the truth, nearly the same ignorance in this respect prevails now as in his time. The decrease of density in ascending is a complicated effect of many causes for the most part unknown; and it seems in vain to expect a satisfactory investigation of it by arbitrary suppositions. But setting aside hypothetical constitutions of the atmosphere, we may consider the rarefaction of the air in ascending as a phænomenon, the knowledge of which is to be acquired by experiment; and this appears the only sure way of placing the theory of the mean refractions on its proper foundation.

5. One of the tables of refraction most esteemed by astronomers is that published annually in the *Con. des Temps.* It has been already shown that, as far as 74° from the zenith, this table is calculated by the simple method of Cassini. There is nothing incidental in this; for all tables of refraction may be computed by Cassini's method to the extent mentioned. The French astronomers have been very successful

in determining the constants of the formula. The refractive power of the air was obtained by Delambre from a great number of astronomical observations; the same quantity was deduced by MM. Biot and Arago from experiments on the gases with the prism; and the results of two methods, so entirely different, agree so nearly, that there seems no ground for preferring one to the other. The remaining part of the French table, for altitudes less than 16°, is computed by a method of Laplace, which the author has explained, without disguising its defects, in the fourth volume of the *Méc. Céleste.* The two atmospheres with densities decreasing in arithmetical and geometrical progression, which it now appears were imagined by Newton, and which have been discussed by Thomas Simpson and other geometers, are found, when the same elements are employed, to bring out horizontal refractions on opposite sides of the observed quantity. Laplace conjectured that an intermediate atmosphere which should partake of the nature of both, and should agree with observation in the horizontal refraction, would approach nearly to the true atmosphere. It must be allowed, that these conditions, which may be verified by innumerable instances between the two limits, are vaguely defined; and in order to ascertain the real meaning of the author, recourse must be had to the algebraic expressions. When this is done, it will be found that the atmosphere intended is one of which the density is the product of two terms, one taken from an arithmetical, and the other from a geometrical progression; the effect of which combination is to introduce a supernumerary constant, by means of which the horizontal refraction is made to agree with the true quantity. No one will deny the merit and the ingenuity of Laplace's procedure; but though very skilful, and guided in some degree by fact, it is liable to all the uncertainty of other arbitrary suppositions, as indeed the author allows. Dr. Brinkley has given the character of the French table fairly when he says, that it is only a little less empirical than the other tables. On divesting Laplace's hypothesis of vagueness in the language, and expressing it in the unequivocal symbols of algebra, it does not appear to possess any superiority over other supposed constitutions of the atmosphere in leading to a better and less exceptionable theory; at least the *Méc. Céleste* has been many years before the public, during which time not a few geometers have laboured on the subject of the refractions; but no improvement originating in the speculations peculiar to Laplace has occurred to any of them.

Having said so much on the theory of the French table, it may be proper to add a word on its accuracy. If it be compared from 80° to 88° of zenith distance with Bessel's observed refractions, there will be found a small error in excess, continually increasing, and amounting at last to 4″. This shows that, in combining the two atmospheres, too much weight has been given to that with a density varying in geometrical progression, in consequence of which the air is not rarefied enough in the interpolated atmosphere. With respect to the two degrees of altitude next the horizon, no accurate judgement can be formed, for want of observed refractions that can be depended on.

The astronomical refractions have also occupied the attention of the astronomer of Kœnigsberg, who has contributed so largely to the improvement of every part of astronomical science. For the purpose of representing the observations of Bradley with all the accuracy possible, Bessel investigated a table of refractions which appeared in the *Fundamenta Astronomiæ* in 1818. He assumes a theoretical formula; but as every arbitrary quantity is determined by a careful comparison with real observations, what is supposititious may be considered merely as an instrument of investigation, which is finally laid aside, leaving the result to rest on the foundation of fact. He returned to the subject in his *Tabulæ Regiomontanæ*, published in 1830. In this last work he retains only that part of the table of 1818 which extends to 85° from the zenith, many corrections being applied from recent observations made with improved instruments. In order to supply what is wanting in the new table, Bessel has added a supplemental one containing the refractions at every half degree for altitudes less than 15°: which supplemental table is independent of theory, being deduced from observations alone. These two tables form together a real table of mean refractions, independent of all suppositions respecting the constitution of the atmosphere; and no other similar table of nearly equal authority is to be found in the astronomy of the present day. What Bessel has accomplished on the subject of the refractions is not the least important part of his labours for the advancement of astronomical science: it is precious to the practical astronomer; and it is necessary to the theoretical inquirer, for enabling him to confront his speculations with the phænomena to be accounted for.

[To be continued.]

LV. *Remarks on a Note in Prof.* Sedgwick *and Mr.* Murchison's *Communication in the last Number. By* John Phillips, *Esq., F.R.S., F.G.S., Professor of Geology in King's College.*

To R. Taylor, Esq.

My dear Sir, York, April 11, 1839.

IN the communication regarding the classification of Devonshire strata by Professor Sedgwick and Mr. Murchison (contained in the last Number of the Philosophical Magazine), I find my name (p. 257, note) connected with a 'promise,' which it appears to me necessary both to explain and to fulfil as soon as possible after the pledge which has been unexpectedly and publicly given for me. This is the more necessary because the statement alluded to does not express completely either the error assigned, or the correction prepared.

There is no '*inaccuracy*' in the acknowledgement engraved on my geological map, that Devonshire is coloured from Mr. De la Beche. On the contrary, it is to Mr. De la Beche, alone, that I am indebted for the means of colouring geologically Cornwall, Devon, and West Somerset. I have, for these districts, merely copied the complete map with which he most kindly furnished me early in the year 1838; nor have I since applied to any person for information on those parts of the country.

The acknowledgement on my map is, however, in one respect *incomplete*; it might have been stated that in colouring as a part of the Carboniferous System the disputed culm deposits of Devonshire, I was following the classification first proposed by Professor Sedgwick and Mr. Murchison at the Bristol Meeting of the British Association. This is the correction, or rather *addition*, which I have promised to make in the next edition of my map.

It is perhaps *unnecessary* to do this, since the brief notices on my map had no other object than to authenticate the *delineation*, and the classification in question was perfectly known to geologists, and secured to the right owners by several publications of two years earlier date*. It is, however, possible, as the authors of this very important change in the systematic distribution of English strata appear to think, that by my silence concerning their labours an erroneous impres-

* See in particular the Sixth Report (1836) of the British Association, p. 95, and Athenæum for 1836, p. 611. The latter contains the whole *discussion* of the subject at the Bristol Meeting of the Association, with a section according to the new views there advocated. A working map was then exhibited by Mr. Murchison, coloured to correspond with the section.

sion may have been communicated to persons not well acquainted with the progress of geological investigation, and I shall gladly embrace the opportunity afforded by a new edition of my map to supply the omission, and to add my unequivocal testimony to the originality and value of the discoveries regarding the sequence of Devonian strata which heralded the new and remarkable views contained in your last Number. I am, my dear Sir, yours, &c.

JOHN PHILLIPS.

LVI. *Supplementary Remarks on the "Devonian" System of Rocks. By the Rev. Professor* SEDGWICK, *F.R.S., F.G.S., and* RODERICK IMPEY MURCHISON, *Esq., F.R.S. F.G.S.**

WE beg to offer a few additional observations on the classification of the older rocks of Devon and Cornwall, as a supplement to our paper in the last Number of this Journal.

In briefly alluding to the geologists who had at different times suggested, that certain portions of these rocks (in South Devon) might be the equivalents of the carboniferous limestone or old red sandstone, we inadvertently omitted to state, that in regard to *North* Devon, our distinguished leader Mr. William Smith had long ago represented† the band of red rocks, which extend along the coast from Combe Martin to the North Foreland, and thence into the Quantock Hills, as old red sandstone. At that early period, however, this author had not the materials for an extension of his first correct glance: neither the order of superposition, nor the fossils of the succeeding strata, were then known; and hence, relying chiefly upon mineral characters, and not upon that great principle, *of identifying strata by their organic remains*, by which he had so successfully eliminated the true order of the *secondary* formations, Smith did not succeed in referring any other part of Devonshire or Cornwall to what we consider its proper place in the geological series. Thus, we find him equally classing as old red sandstone, a course of red rocks which may be traced across a considerable part of Devonshire, a little south of Bideford, and which we have shown to be a mass included in the culm or *coal measures* of the county. Again, he placed the limestones of Holcombe Rogus, &c., which we also include in the same carboniferous tract, as subordinate to the *new* red sandstone; and in common with all who preceded or followed him up to the period when we published our views in 1836, he considered the black

* Communicated by the Authors.

† Smith's Geological Map of England and Wales.

culm measures, and the slates of South Devon and Cornwall, to be among the *oldest transition rocks.* Still the first indications of a more accurate view, as given in his representation of the North Coast of Devon, is to us a strong proof both of his boldness and sagacity. Doubtless deceived by mineral characters, slaty cleavage, and the antique impress of many parts of this region, he never thought of applying his own sound principles to Devonshire, still less to the killas and so called "primary" rocks of Cornwall. Yet is it entirely through the application of Smith's "Strata identified by their organic remains," that we have been enabled at length to work out what we believe to be a correct classification of these rocks, and to place them so much higher in the series than had ever been contemplated by those who have gone before us. The imbedded plants and shells, and the strong analogy of the culm strata of Devonshire to those which we had studied in Pembrokeshire, coupled with their *overlying* position, first led us to venture to assert that so large a portion of this region was truly an equivalent of our ordinary coal fields: and assuming that our view has been sustained, we have since shown, upon the same principles of imbedded fossils and a conformable succession of strata, that all the rocks which rise from beneath the great culm basin (with perhaps some slight exceptions), whether slates, killas, limestone or sandstone, form one great natural group (hitherto called old red sandstone), which occupies the great interval between the Carboniferous System above, and the Silurian System below it; each of these three great systems being defined, as we have before explained, by a characteristic series of organic remains.

We have much pleasure in stating that, since the appearance of our memoir in the last Number of this Journal, the reading of a paper by the Rev. D. Williams to the Geological Society upon the structure of Devonshire, gave rise to a discussion, during which certain explanations took place which have happily removed from our minds any tendency to suppose, that our fellow-labourers Mr. De la Beche and Mr. Williams were unwilling to accord to us the merit (whatever it may be) of having been the first to propose a great change in the grouping and classification of the rocks of Devonshire and Cornwall. We beg, therefore, to say, that Mr. De la Beche having in a kind and friendly manner read from his Report the passage quoted below*, and having assured the geo-

* "In 1836, Prof. Sedgwick and Mr. Murchison read a memoir before the meeting of the British Association held that year at Bristol, in which

logists present, that he meant that passage fully to convey the meaning, that we were the first persons who proposed, on the evidence of superposition as well as fossils, that the large tract of Devon and Cornwall (now represented in his map as "Carbonaceous Rocks") should be considered as a distinct physical group belonging to the Carboniferous System; we are on our part anxious, not only to state publicly, that we are satisfied of the entire sincerity of this assurance, but also to express (as one of us did at the Geological Society) our deep regret that one or two words were employed in our memoir which gave pain to our associate. In short, there does not now exist the slightest misunderstanding between Mr. De la Beche and ourselves.

And next in regard to the Rev. D. Williams, to whom we adverted as putting forth a claim to have been the first to indicate the existence of a trough in Central Devon, we find (having been favoured by him with a sight of the original notice which he read at Dublin 1835) that the sole expression which, according to himself, bears upon the point is this: "The argillaceous schist reposes immediately on the granite of Lundy Island, mantling round its south-eastern angle on the one hand and dipping away towards it from the granite of Dartmoor on the other." Mr. Williams indeed allows, that he did not then even mention the word "trough," nor was there, indeed, a single expression in the short notice which could have led any one to suppose that he had then discovered, that two-thirds of the large county of Devon consisted of a great culmiferous basin overlying older strata, *still less* that this basin was the equivalent of our *coal fields*. For his observations in that and all his subsequent memoirs (even that just read) go to maintain, that these culm strata are of an age quite distinct from the Carboniferous æra, and form an integral part of the transition series of a much older epoch.

But in asserting the priority and independence of our own views, and in pointing out how much they differ from those which Mr. De la Beche, Mr. Williams, or indeed any geologists had previously entertained, we are bound to state, that

they separated the upper mass of rocks of North Devon which had hitherto been termed grauwacke from the lower, stated that these rocks rested in a trough-like cavity extending east and west, and concluded that they had been improperly classed with the lower series, inasmuch as they considered these rocks *to be equivalent to the beds usually termed coal-measures*. The inferior rocks they divided into five subordinate groups." In the same page Mr. De la Beche also states the substance of our subsequent paper (1837) read before the Geological Society (See Report, p. 43.).

we believe the observations of Mr. Williams to be also equally original; for he has recently shown to us a MS. which he sent to the Local Secretaries of the meeting of the British Association for the Advancement of Science, held at Bristol (which arrived *after* the memoir by ourselves was read, and on that account was not brought forward), in which he expresses his opinion that the contorted carbonaceous rocks upon the coast between North Devon and Cornwall, occupy a trough supported at either extremity on the west coast, by rocks of older age; the whole, however, belonging to an ancient transition period, and the culm strata being of "far higher antiquity than the carboniferous series proper." And we may further remark, that at that time he had neither separated (as a distinct formation) the black culmiferous limestone from the inferior courses of limestone of North Devon, nor ascertained the fact of its re-appearance on the south side of the culm trough and on the northern flank of Dartmoor. From an inspection of the map of Devonshire which Mr. Williams is preparing, and the sections which were exhibited with it at the Geological Society, we are convinced he is an industrious observer; while the richness of his fossil collections also manifests his diligence and zeal: his collections are, indeed, among the most valuable (in respect to North Devon and Cornwall) which have come under our notice; and their examination, to which he has cordially invited us, has been a source of much gratification, as it offers a strong confirmation of our opinion of the age of the Devonian and Cornish strata.

There is now, therefore, no longer any strife concerning the structure of this region: a generous rivalry alone exists to bring to the common stock of knowledge all the proofs by which the true age of these hitherto anomalous strata can be determined. In fact, the order of superposition of the region is completely settled, for the principal sections of Mr. De la Beche and Mr. Williams are *now* in accordance with our own, in presenting the same succession of mineral masses; and it only remains that we should enter into a hearty union to determine the precise coordinates of these masses by a thorough examination of their imbedded organic remains.

We have already stated in the previous notice, and we have sincere pleasure in repeating it, that we owe deep obligations to our friend Mr. Lonsdale for having first suggested to us that the Devon fossils would be found to be those of the old red system; a point which he was admirably qualified to determine from having compared so closely the fossils (particularly the corals) of the Carboniferous and Silurian Systems.

If therefore, as we now believe, (together with Mr. Lonsdale and Mr. James Sowerby) that these peculiar fossils constitute the links which connect the Carboniferous and Silurian Systems, we have only to produce clear evidences; and this done, we venture to assert, that however indisposed *a priori*, to admit the necessity of so great and sweeping a reform, there is no lover of truth (and all true geologists are such) who will not participate in the conviction, that as a few important pages in the early history of the earth have been unfolded for the first time in Devonshire, we are entitled to suggest that the "Devonian System" be henceforth a term admitted into our geological nomenclature.

P.S. In addition to what we have written in this Journal, we have prepared for reading before the Geological Society, a short exposé of our present views concerning the structure of Devon and Cornwall, accompanied by a geological map of the West of England and Wales, which illustrates the amount of change proposed in this new classification. It is also our intention to visit in the course of this summer those districts of France (Britany, &c.) in which the existence of transition coal fields has been maintained, as we deem it highly probable, that the application of the same principles which have enabled us to classify the rocks of Devon and Cornwall, may be successfully employed in that region of France which seems to be physically connected with the country in question.

April 19, 1839.

LVII. *On the Classification of certain Geological Formations in Devonshire. By the Rev.* David Williams, *F.G.S.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

FEELING assured that Prof. Sedgwick and Mr. Murchison will do me justice with respect to as much of the article in your last Number which implicates me more seriously than any one else, I trouble you only with a few remarks on the hypothesis of Mr. Lonsdale, which they have reanimated and quickened by a new sanction and impulse. Much I apprehend will depend on future observations before it can with justice be received or rejected. I always believed the plant and culm-bearing rocks to belong to the upper grauwacke; and if the carboniferous champions will only modify their views a little, and include *the whole* in their old red sandstone group, I know of no difficulty at present to my adopting the theory; but I must unlearn a great deal before I admit the

carbonaceous limestones and fossiliferous rocks and culm of Devon and Cornwall to be true coordinates of the upper great coal-field and its carboniferous limestone. At a late meeting of the Geological Society, I endeavoured to show that these black limestones, containing Goniatites, Posidoniæ, &c. were in their eastward extension, viz. at Holcomb Rogus, Chudleigh and Ashburton, overlaid by the gray coralline limestones of those places, and I entertain no doubt that the greater part of their zoophytous reliquiæ will be shown not to appertain to the mountain limestone. All the prominent phænomena of the county tend to show that we are here first ascending within the carboniferous influence, or perhaps attaining the rudimentary efforts of nature at a coal deposit. The downward tendency of the coal in the North of England below the carboniferous limestone, as shown by Professor Sedgwick, and among the old red sandstone in Scotland, as shown by Mr. Murchison, encourages me somewhat in being the bearer of this flag of truce. If the sum of evidence adduced hereafter should prove these culmiferous limestones not to be an equivalent for the mountain limestone, why they must be below it; and the mountain limestone, millstone grit, and the upper great coal-field are not represented here at all, inasmuch as the Devonshire culm measures are inseparably associated with their limestones and subordinate slate rocks, by an indisputable transition and a perfect conformity of strike and dip. The base line of the carboniferous system would thus be placed at a lower level as suggested by Prof. Sedgwick at Liverpool, and the correctness of Mr. De la Beche's original views of the grauwacke age of the plants and culm admitted. I remain, Gentlemen, yours, &c.

Bleadon W. Cross, April 24, 1839. D. WILLIAMS.

LVIII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

Feb. 14, 1839. A Paper was read, entitled, "Researches on the Chemical Equivalents of certain Bodies." By Richard Phillips, Esq., F.R.S.

The author examines, by a new series of experiments, the truth of the theory of Dr. Prout and Dr. Thomson, namely, that "all atomic weights are simple multiples of that of hydrogen," a theory which the late Dr. Turner had maintained is at variance with the most exact analytic researches, and consequently untenable. Although the experiments of Dr. Turner, and the inferences which he drew from them, agree very nearly with those of Berzelius, it still appeared to the author desirable to investigate this subject; and it

occurred to him that the inquiry might be conducted in a mode not liable to some of the objections which might be urged against the processes usually employed.

Dr. Turner having adopted a whole number, namely 108, as the equivalent of silver, this substance was selected by the author as the basis of his inquiry into the equivalent numbers of chlorine, and some other elementary gases. It appeared to him that the chance of error arising from the fusing of the chloride of silver might be entirely removed, and other advantages gained, by experimenting on silver on a large scale, with such proportions of the substances employed as were deemed to be equivalents; and instead of calculating from the whole product of the fused chloride, to do it merely from the weight of such small portion only, as might arise from the difference between theoretical views and experimental results.

The author concludes, from the train of reasoning he applies to the series of experiments so undertaken, that no material, and even scarcely any appreciable error can arise from considering the equivalent numbers of hydrogen, oxygen, azote, and chlorine, as being 1, 8, 14, and 36 respectively.

A paper was also read, entitled, "Some Account of the Hurricane of the 7th of January, 1839, as it was experienced in the neighbourhood of Dumfries," in a letter addressed to P. M. Roget, M.D. Secretary to the Royal Society. By P. Garden, Esq. Communicated by Dr. Roget.

After describing the position of his house, and the nature of the instruments employed for observation, the writer gives his observations of the barometer and thermometer on the 6th and 7th of January last, and proceeds to state, that on the 6th, at about ten minutes past ten o'clock p.m. violent squalls commenced, at first with intermissions of perfect calms, but gradually becoming more frequent, and being accompanied by the sound of strong and increasing whirlwinds. By eleven o'clock the wind was observed to proceed from the East, and its velocity was estimated at forty miles an hour. Its violence then increased, and threatened to blow down the chimneys. At midnight it abated, at the same time shifting to the south or west. At two o'clock in the morning nearly two tons of lead were torn away by the wind from the west end platform on the house-top, and thrown down behind the house in a westerly direction. Some of the lower windows having been left a little open, the wind thus admitted into the house forced up and blew off the very heavy hatch-door of the roof, which was covered with lead. The whole house rocked terribly, and even the stone floor of the half-sunk kitchen story heaved as if shaken by an earthquake: the slates from the roof were blown in every direction, some being carried to a prodigious distance. During the greater part of the night the rain fell in tremendous torrents. In the interval from two to half-past three in the morning, the barometer sunk very nearly an inch and a half, and reached its greatest depression. But the tempest continued till about four o'clock, when it began gradually to subside. Extensive devastation occurred among the trees; some that

were blown down raising two or three tons of clay soil with the roots. Several trees thus thrown down fell with their tops to the W.N.W.

The writer concludes from these and other observations, that the first and squally part of the storm began from the E.S.E., and blew from S. by W. at about midnight; and that most injury was done to the slating and roof when the wind was not far from the south. It then gradually veered to the west, till noon, and reached the N.W. point by eight o'clock in the evening of the same day.

Feb. 21.—A paper was read, entitled, "An Account of the Processes employed in Photogenic Drawing," in a letter to S. Hunter Christie, Esq., Sec. R.S. By H. Fox Talbot, Esq., F.R.S.*

A paper was also read, entitled, "A Description of a Hydropneumatic Baroscope." By J. T. Cooper, Esq., Lecturer on Chemistry.

The liability of the ordinary mercurial barometer to derangement and to fracture, led the author to the construction of an instrument for measuring atmospheric pressure that should be exempt from these objections. It consists of a float, formed by a brass tube, having the shape of the frustum of an inverted cone, nine inches long, two inches in diameter above, and one inch below, and its content being about fourteen cubic inches. From the centre of the upper and wider end, which is closed, a brass wire proceeds, surmounted by a cup, for the purpose of holding such weights as are necessary for bringing the float, when immersed in water, to the same constant level. The lower and smaller end of the tube is closed by a brass plug, sufficiently heavy to sink the instrument to the proper depth, and maintain its position, and having a small perforation in its centre. This float is inclosed in a case, containing the water in which it is to be immersed, and which is to be raised to a constant given temperature by a spirit lamp burning beneath it. The float being first filled with water, a given portion of this water is poured out into a measure of known capacity, and is consequently replaced by an equal volume of air, the dilatations or contractions of which will, when the temperature is constant, be dependent only on the external pressure of the atmosphere; and the latter will, therefore, be indicated by the weights requisite to be placed in the cup of the float, in order to maintain it at the same level in the fluid, on the principle of the hydrometer. The author gives a minute description of all the parts of the apparatus, of the method of using it, and of the adjustments and calculations required for determining by its means the difference of level of two stations.

Mr. Darwin's paper, entitled, "On the Parallel Roads of Glen Roy, and other parts of Lochaber, &c.," was resumed, but not concluded.

Feb. 28.—The reading of a paper, entitled, "Observations on the Parallel Roads of Glen Roy, and of other parts of Lochaber, with an attempt to prove that they are of Marine Origin." By Charles Dar-

* This paper was given entire in our Number for March; pres. vol. p. 209.—Edit.

win, Esq., M.A., F.R.S., Sec. Geological Society, was resumed and concluded.

The author premises a brief description of the parallel roads, shelves, or lines, as they have been indefinitely called, which are most conspicuous in Glen Roy and the neighbouring valleys, referring for more detailed accounts to those given by Sir Thomas Lauder Dick, in the Transactions of the Royal Society of Edinburgh, and by Dr. Macculloch in those of the Geological Society of London. Both these geologists endeavour to explain the formation of these shelves on the hypothesis of their resulting from depositions at the margin of lakes, which had formerly existed at those levels. The author, however, shows that this hypothesis is inadmissible, from the insuperable difficulties opposed to any conceivable mode of the construction and removal, at successive periods, of several barriers of immense size, whether placed at the mouths of the separate glens, or at more distant points. He does not, however, propose the alternative, that the beaches, if not deposited by lakes, must of necessity have been formed by channels of the sea, because he deems it more satisfactory to prove, from independent phenomena, that a sheet of water, gradually subsiding from the height of the upper shelves to the present level of the sea, occupied for long periods not only the glens of Lochaber, but the greater number, if not all the valleys of that part of Scotland; and that this water must have been that of the sea. It is argued by the author, that the fluctuating element must have been the land, from the ascertained fact of the land rising in one part, and at the same time sinking in another; and therefore, that this change of level in Scotland, attested as it is by marine remains being found at considerable heights both on the eastern and western coasts, implies the elevation of the land, and not the subsidence of the surrounding waters. The author next shows, that in all prolonged upward movements of this kind, it might be predicted, both from the analogy of volcanic action, and from the occurrence of lines of escarpment rising one above the other in certain regions, that in the action of the subterranean impulses there would be intervals of rest. On the hypothesis that the land was subjected to these conditions, it appears that its surface would have been modeled in a manner exactly similar, even in its minute details, to the existing structure of the valleys in Lochaber. Considering that he has thus established his theory, the author proceeds to remove the objections which might be urged against its truth, derived from the non-extension of the shelves, and the absence of organic remains at great altitudes. He then shows how various details respecting the structure of the glens of Lochaber, such as the extent of corrosion of the solid rock, the quantity of shingle, the numerous levels at which water must have remained, the forms of the heads of the valley, where the streams divide, and especially their relation with the shelves, and the succession of terraces near the mouth of Glen Spean, are all explicable on the supposition that the valleys had become occupied by arms of a sea which had been subject to tides, and which had gradually subsided during

the rising of the land; two conditions which could not be fulfilled in any lake. From the attentive consideration bestowed by the author on these several and independent steps of the argument, he regards the truth of the theory of the marine origin of the parallel roads of Lochaber (a theory, of which the foundation stone may be said to have been laid by the important geological researches of Mr. Lyell, establishing the fact of continents having slowly emerged from beneath the sea) as being sufficiently demonstrated.

The author states, in the concluding part of his paper, the following as being the chief points which receive illustration from the examination of the district of Lochaber by Sir Thomas Lauder Dick, Dr. Macculloch, and himself. It appears that nearly the whole of the water-worn materials in the valleys of this part of Scotland were left, as they now exist, by the slowly retiring waters of the sea; and the principal action of the rivers since that period has been to remove such deposits; and when this had been effected, to excavate a wall-sided gorge in the solid rock. Throughout this entire district, every main, and most of the lesser inequalities of surface are due, primarily to the elevating forces, and, secondarily, to the modeling power of successive beach-lines. The ordinary alluvial action has been exceedingly insignificant; and even moderately sized streams have worn much less deeply into the solid rock than might have been anticipated, during the vast period which must have elapsed since the sea was on a level with the upper shelves: even the steep slopes of turf over large spaces, and the bare surface of certain rocks, having been perfectly preserved during the same lapse of time. The elevation of this part of Scotland to the amount of at least 1278 feet was extremely gradual, and was interrupted by long intervals of rest. It took place either during the so called "erratic block period," or afterwards; and it is probable that the erratic blocks were transported during the quiet formation of the shelves. One of these was found at an altitude of 2200 feet above the present level of the sea. The most extraordinary fact is, that a large tract of country was elevated to a great height, so equably, that the ancient beach-lines retain the same curvature, or nearly so, which they had when forming the margin of the convex surface of the ancient waters. The inferences drawn by the author from these facts, and which he corroborates by other evidence, are that a large area must have been uplifted, and that its rise was effected by a slight change in the convex form of the fluid matter on which the crust of the earth rests; and therefore that the fluidity of the former is sufficiently perfect to allow of the atoms moving in obedience to the law of gravitation, and consequently, of the operation of that law modified by the centrifugal force: and lastly, that even the disturbing forces do not tend to give to the earth a figure widely different from that of a spheroid in equilibrium.

March 7.—A paper was read, entitled, "On the Male Organs of some of the Cartilaginous Fishes." By John Davy, M.D., F.R.S., Assistant Inspector of Army Hospitals.

In this paper, which is wholly occupied with anatomical details,

the author refers to his paper on the Torpedo, which was published in the Philosophical Transactions for 1834; and also to Müller's work "De Glandularum secernentium structura penitiori," whose descriptions and views are not in accordance with those given in that paper. In the present memoir he adduces evidence of the accuracy of his former statement, chiefly founded on microscopical observations, and offers some conjectures respecting the functions of several organs found in cartilaginous fishes; but does not pretend to attach undue importance to his speculations.

A paper was also read, entitled, "Researches in Physical Geology.—*Third Series*. On the Phenomena of Precession and Nutation, assuming the interior of the earth to be a heterogeneous fluid." By W. Hopkins, Esq., M.A., F.R.S., &c.

Having, in his last memoir, completed the investigation of the amount of precession and nutation, on the hypothesis of the earth's consisting of a homogeneous fluid mass, contained in a homogeneous solid shell, the author here extends the inquiry to the case in which both the interior fluid and external shell are considered as heterogeneous. After giving the details of his analytical investigation, he remarks, that he commenced the inquiry in the expectation that the solution of this problem would lead to results different from those previously obtained on the hypothesis of the earth's entire solidity. This expectation was founded on the great difference existing between the direct action of a force on a solid, and that on a fluid mass, in its tendency to produce a rotatory motion; for, in fact, the disturbing forces of the sun and moon do not tend to produce directly any motion in the interior fluid, in which the rotatory motion causing precession and nutation is produced indirectly by the effect of the same forces on the position of the solid shell. A modification is thus produced in the effects of the centrifugal force, which exactly compensates for the want of any direct effect from the action of the disturbing forces; a compensation which the author considers as scarcely less curious than many others already recognized in the solar system, and by which, amidst many conflicting causes, its harmony and permanence are so beautifully and wonderfully preserved.

The solution of the problem obtained by the author destroys the force of an argument, which might have been urged against the hypothesis of central fluidity, founded on the presumed improbability of our being able to account for the phenomena of precession and nutation on this hypothesis, as satisfactorily as on that of internal solidity. The object, however, of physical researches of this kind is not merely to determine the actual state of the globe, but also to trace its past history through that succession of ages, in which the matter composing it has probably passed gradually through all the stages between a simple elementary state and that in which it has become adapted to the habitation of man. In this point of view the author conceives the problem he proposes is not without value, as demonstrating an important fact in the history of the earth, presuming its solidification to have begun at the surface; namely, the permanence of the inclination of its axis of rotation, from the epoch

of the first formation of an exterior crust. This permanence has frequently been insisted on, and is highly important as connected with the speculations of the author on the causes of that change of temperature which has probably taken place in the higher latitudes: all previous proofs of this fact having rested on the assumption of the earth's entire solidity; an assumption which, whatever may be the actual state of our planet, can never be admitted as applicable to it at all past epochs of time, at which it may have been the habitation of animate beings.

The author concludes, by expressing a hope that he may be enabled to prosecute the inquiry still further, and to bring before the Royal Society, at a future time, the matured results of his speculations.

March 14.—A paper was read, entitled, "An Experimental Inquiry into the Formation of Alkaline and Earthy Bodies, with reference to their presence in Plants, the Influence of Carbonic Acid in their generation, and the equilibrium of this gas in the atmosphere." By Robert Rigg, Esq. Communicated by the Rev. J. B. Reade, M,A., F.R.S.

The object of the author, in the present memoir, is to show that the solid materials which compose the residual matter in the analysis of vegetable substances, and which consist of alkaline and earthy bodies, are actually formed during the process of fermentation, whether that process be excited artificially, by the addition of a small quantity of yeast to fermentable mixtures, or take place naturally in the course of vegetation, or of spontaneous decomposition. His experiments also tend to show that this formation of alkaline and earthy bodies is always preceded by the absorption of carbonic acid, whether that acid be naturally formed or artificially supplied. He finds, also, that different kinds of garden mould, some being calcareous, others siliceous, and others aluminous, exposed in retorts to atmospheres consisting of a mixture of carbonic acid gas and common air, absorb large quantities of the former, combining with it in such a manner as not to afford any traces of this carbonic acid being disengaged by the action of other acids. He considers the result of this combination to be the formation of an alkaline body, and also of a colouring matter. This combination takes place to a greater extent during the night than during the day; and in general, the absorption of carbonic acid by the soil is greatest in proportion as it is more abundantly produced by the processes of vegetation; and conversely, it is least at the time when plants decompose this gas, appropriating its basis to the purposes of their own system. Hence he conceives that there is established in nature a remarkable compensating provision, which regulates the quantity of carbonic acid in the atmosphere, and renders its proportion constant.

A paper was also read, entitled, "Note on the Art of Photography, or the application of the Chemical Rays of Light to the purposes of Pictorial Representation." By Sir John F. W. Herschel, Bart., K.H., V.P.R.S., &c.

The author states that his attention was first called to the subject of M. Daguerre's concealed photographic processes, by a note from

Captain Beaufort, dated the 22nd of January last, at which time he was ignorant that it had been considered by Mr. Talbot, or by any one in this country. As an enigma to be solved, a variety of processes at once presented themselves, of which the most promising are the following; 1st, the so-called de-oxidizing power of the chemical rays in their action on recently precipitated chloride of silver; 2ndly, the instant and copious precipitation of a mixture of a solution of muriate of platina and lime-water by solar light, forming an insoluble compound, which might afterwards be blackened by a variety of agents; 3rdly, the reduction of gold in contact with de-oxidizing agents; and, 4thly, the decomposition of an argentine compound soluble in water, exposed to light in an atmosphere of peroxide of chlorine, either pure or dilated.

Confining his attention, in the present notice, to the employment of chloride of silver, the author inquires into the methods by which the blackened traces can be preserved, which may be effected, he observes, by the application of any liquid capable of dissolving and washing off the unchanged chloride, but of leaving the reduced, or oxide of silver, untouched. These conditions are best fulfilled by the liquid hyposulphites. Pure water will fix the photograph, by washing out the nitrate of silver, but the tint of the picture resulting is brick-red; but the black colour may be restored by washing it over with a weak solution of hyposulphite of ammonia.

The author found that paper impregnated with the chloride of silver was only slightly susceptible to the influence of light: but an accidental observation led him to the discovery of other salts of silver, in which the acid being more volatile, adheres to the base by a weak affinity, and which impart much greater sensibility to the paper on which they are applied; such as the carbonate, the nitrate, and the acetate. The nitrate requires to be perfectly neutral; for the least excess of acid lowers in a remarkable degree its susceptibility.

In the application of photographic processes to the copying of engravings or drawings, many precautions, and minute attention to a number of apparently trivial, but really important circumstances, are required to ensure success. In the first tranfers, both light and shadow, as well as right and left, are the reverse of the original: and to operate a second transfer, or by a double inversion to reproduce the original effect, is a matter of infinitely greater difficulty; and in which the author has only recently ascertained the cause of former failures, and the remedy to be applied.

It was during the prosecution of these experiments that the author was led to notice some remarkable facts relating to the action of the chemical rays. He ascertained that, contrary to the prevailing opinion, the chemical action of light is by no means proportional to the quantity of violet rays transmitted, or even to the general tendency of the tint to the violet end of the spectrum: and his experiments lead to the conclusion that, in the same manner as media have been ascertained to have relations *sui generis* to the calorific rays, not regulated by their relations to the rays of illumination and of colour, they have also specific relations to the chemical spectrum,

different from those they bear to the other kinds of spectra. For the successful prosecution of this curious investigation, the first step must consist in the minute examination of the chemical actions of all the parts of a pure spectrum, not formed by material prisms, and he points out, for that purpose, one formed in Fraunhofer's method, by the interference of the rays of light themselves in passing through gratings, and fixed by the heliostat.

He notices a curious phenomenon respecting the action of light on nitrated paper; namely, its great increase of intensity, under a certain kind of glass strongly pressed in contact with it; an effect which cannot be explained either by the reflection of light, or the presence of moisture; but which may possibly be dependent on the evolution of heat.

Twenty-three specimens of photographs, made by Sir John Herschel, accompany this paper: one, a sketch of his telescope at Slough, fixed from its image in a lens; and the rest copies of engravings and drawings, some reverse, or first transfers; and others second transfers or re-reversed pictures.

March 21.—The following papers were read:—

I. "Description of a Compensating Barometer, adapted to Meteorological purposes, and requiring no corrections either for Zero, or for Temperature." By Samuel B. Howlett, Esq., Chief Military Draftsman, Ordnance. Communicated by Sir John F. W. Herschel, Bart., K.H., V.P.R.S., &c.

In the instrument here described, there is provided, in addition to the ordinary barometric tube (inverted, in the usual way, in a cistern of mercury,) a second tube of the same dimensions, placed by the side of the former, and likewise filled with mercury, but only to the height of twenty-eight inches above the level of the mercury of the cistern. This tube is closed at its lower end, and fixed to a float supported by the mercury in the cistern: and it bears, at its upper end, an ivory scale, three inches in length. The elevation of the mercury in the barometric tube is estimated by the difference between its level and that of the mercury in the closed tube; and is measured on the ivory scale by the aid of a horizontal index, embracing both the tubes, and sliding vertically along them. As the float which bears the closed tube, to which the scale is attached, rests freely on the mercury in the cistern, and consequently always adjusts itself to the level of that fluid, no correction for the zero point is needed; and as every change of temperature must similarly affect the columns of mercury in both the tubes, after the scale has been adjusted so as to read correctly at any given temperature, such as 32°, which may be effected by comparison with a standard barometer, every other reading will correspond to the same temperature, and will require no correction. The author considers the error arising from the difference of expansion corresponding to the different lengths of the two columns of mercury, and which will rarely amount to one four-hundredth of an inch, as too small to deserve attention in practice, being, in fact, far within the limits of error in ordinary observations.

Subjoined to the above paper is a letter from the author to Sir John Herschel, containing a statement of comparative observations made with a mountain barometer, and with the compensation barometer, from which it appears that the use of the latter is attended with the saving of a great quantity of troublesome calculation. The comparative observations are given in a table, exhibiting a range of differences from +·012 to −·016 of an inch.

II. "An Account of the fall of a Meteoric Stone in the Cold Bokkeveld, Cape of Good Hope." By Thomas Maclear, Esq., F.R.S., &c., in a letter to Sir John F. W. Herschel, Bart., V.P.R.S., and communicated by him.

The appearance attending the fall of this aerolite, which happened at half-past nine o'clock in the morning of the 13th of October, 1838, was that of a meteor of a silvery hue, traversing the atmosphere, for a distance of about sixty miles, and then exploding with a loud noise, like that from artillery, which was heard over an area of more than seventy miles in diameter; the air at the time being calm and sultry. The fragments were widely dispersed; and were at first so soft as to admit of being cut with a knife; but they afterwards spontaneously hardened. The entire mass of the aerolite is estimated at about five cubic feet.*

III. "Chemical Account of the Cold Bokkeveld Meteoric Stone." By Michael Faraday, Esq., D.C.L., F.R.S., &c., in a letter to Sir John F. W. Herschel, Bart., V.P.R.S., &c., and communicated by him.

The stone is stated as being soft, porous, and hygrometric; having when dry, the specific gravity of 2·94; and possessing a very small degree of magnetic power irregularly dispersed through it. One hundred parts of the stone, in its natural state, was found to consist of the following constituents; namely,

Water	6·5	Alumina........	5·22
Sulphur	4·24	Lime	1·64
Silica	28·9	Oxide of Nickel..	·82
Protoxide of Iron....	33·22	Oxide of Chromium	·7
Magnesia..........	19·2	Cobalt and Soda..	a trace.

IV. "Note respecting a new kind of Sensitive Paper." By Henry Fox Talbot, Esq., F.R.S.

The method of preparing the paper here referred to consists in washing it over with nitrate of silver, then with bromide of potassium, and afterwards again with nitrate of silver; drying it at the fire after each operation. This paper is very sensitive to the light of the clouds, and even to the feeblest daylight.

The author supplies an omission in his former memoir on photogenic drawing, by mentioning a method he had invented and practised nearly five years ago, of imitating etchings on copper plate, by smearing over a sheet of glass with a solution of resin in turpentine, and blackening it by the smoke of a candle. On this blackened surface a design is made with the point of a needle, the lines of which

* Some further information respecting this fall of meteorites will be found in the Intelligence and Miscellaneous Articles in the present Number, p. 391.—E. W. B.

will of course be transparent, and will be represented by dark lines on the prepared paper to which it is applied, when exposed to sunshine. The same principle may be applied to make numerous copies of any writing.

The Society then adjourned over the Easter Recess, to meet again on the 11th of April.

LINNÆAN SOCIETY.

March 6, 1838.—Mr. Newman, F.L.S., exhibited specimens of the *Noctua cubicularis* in the larva state, obtained from Ham Green, near Bristol, the seat of Richard Bright, Esq., where this caterpillar had proved very destructive to wheat in the rick.

Dr. Bromfield, F.L.S., exhibited a specimen of a singular variety of *Crepis virens*, with the leaves nearly entire, gathered by him in a wood near Yarmouth in the Isle of Wight.

A plant in flower of the rare *Ophrys lutea* of Cavanillas was exhibited by Mr. Anderson, from the Apothecaries' Garden at Chelsea.

Read a description of the Mosses collected in the journey of the late deputation into Upper Assam, in the years 1835 and 1836. By William Griffith, Esq., Assistant Surgeon on the Madras Establishment. Communicated by R. H. Solly, Esq., F.R.S. & L.S.

March 20, 1838.—Read a description of the Mora tree. By Mr. Robert H. Schomburgk. Communicated by George Bentham, Esq., F.L.S.

April 3, 1838.—Read a communication on the existence of Stomata in Mosses. In a letter to R. H. Solly, Esq., F.R.S. & L.S. By William Valentine, Esq., F.L.S.

Mr. Anderson, F.L.S., exhibited, from the Chelsea Botanic Garden, flowering plants of *Pterostylis concinna* and *Perdicium lyratum*.

April 17, 1838.—Read a paper by Prof. Don, Libr. L.S., describing two new genera of the natural family of plants called *Coniferæ*.

May 1, 1838.—Mr. Curtis read a paper, being descriptions of the *Coleoptera* collected by Capt. P. P. King, R.N., during his survey of the Straits of Magellan.

Read a paper on the affinities of *Arachis* and *Voandzeia*. By George Bentham, Esq., F.L.S.

May 24, 1838.—This day, the anniversary of the birth-day of Linnæus, and that appointed in the charter for the election of Council and Officers, the Right Rev. the Bishop of Norwich, President of the Society, opened the business of the meeting, and in stating the number of Fellows whom the Society had lost during the past year, gave biographical notices of some of them.

At the election which subsequently took place, the Lord Bishop of Norwich was re-elected President; Edward Forster, Esq., Treasurer; Francis Boott, M.D., Secretary; and Richard Taylor, Esq., Under Secretary. The following five gentlemen were elected into the Council in the room of others going out: viz. Arthur Aikin, Esq.; John Jos. Bennett, Esq.; George Bentham, Esq.; the Earl of Derby; and John Guillemard, Esq.

June 5, 1838.—Read observations on the *Spongilla fluviatilis.* By John Hogg, Esq., M.A., F.L.S.

Read also a paper, entitled, on the Number and Structure of the Mammulæ employed by Spiders in the process of Spinning. By John Blackwall, Esq., F.L.S.

Mr. Saunders, F.L.S., presented specimens of *Potamogeton plantagineus* and *Mendicago denticulata, var. apiculata,* gathered in Sussex.

Mr. Hogg, F.L.S., exhibited specimens of *Plumatella repens* and *Spongilla fluviatilis* from a rivulet near Norton in the county of Durham. One of the *Spongillæ* was attached to the larva-case of *Phryganea,* and another to a tuft of *Hypnum riparium,* which it had entirely enveloped.

June 19, 1838.—Specimens of the tree which yields the Caoutchouc or India Rubber of Commerce, and which proves to be a species of *Hevea,* nearly related to the *guianensis* of Aublet, were presented by Sir Everard Home, Bart., Capt. R.N.

Read a Description of a new species of *Cattleya.* By Mr. Robert H. Schomburgk. Communicated by the Secretary.

Read likewise observations on some genera of Plants connected with the Flora of Guiana. By George Bentham, Esq., F.L.S.

Nov. 6, 1838.—Read a letter from Mr. Jonathan Couch, F.L.S., giving an account of a single specimen of Wilson's Petrel (*Procellaria Wilsoni*) having been found dead in a field near Polperro in Cornwall, about the middle of August last, at a time when the Stormy Petrel (*P. pelagica*) abounded on the coast, most probably driven thither by the state of the weather at that period.

Read also Observations on the Cause of Ergot. By Mr. John Smith, A.L.S.

The Chairman announced to the Meeting that the late Nathaniel John Winch, Esq., of Newcastle-upon-Tyne, had bequeathed to the Society his entire Herbarium, consisting of upwards of 12,000 species of plants, together with his library of Natural History.

November 20, 1838.—Read the Description of a new Genus of Plants belonging to the Natural Family *Bignoniaceæ.* By Professor Don, Libr. L.S.

There was also read an account of a new species of *Lepidosperma.* By Dr. John Lhotsky.

GEOLOGICAL SOCIETY.

Feb. 6.—A paper "On a probable Cause of certain Earthquakes," by M. Louis Albert Necker, For. Mem. G. S., was read.

The object of this memoir is to show, that some earthquakes may be due to the falling in of the roof of cavities, produced by the solvent or erosive powers of subterranean bodies of water on beds and masses of gypsum, rock salt, limestone, marl, clay or sand.

M. Necker was induced to enter upon the inquiry in consequence of the earthquake which desolated, in 1829, a considerable part of the country on the banks of the Segura, in Murcia, having occurred in a district, which is stated to contain no volcanic or trappean rocks; and because the event was unaccompanied by any of

those phenomena which, he conceives, precede, attend, or follow true volcanic earthquakes.

Of the places where earthquakes have been felt without there being any traces of volcanic or trap rocks, but where gypsum is known to occur, and in which, from that mineral being, in his opinion, of comparatively easy renewal, he supposes, caverns exist, M. Necker more particularly mentions Bâle, Nice, Navarroux, Oleron, Maulen, Bagnorre de Bigorre, and the Gave Maulen, in the Pyrenees; he also alludes to the shocks which were felt at Clanssaye, near St. Paul-trois-Chateaux, in the department of the Drome, from the 1st of June, 1772, to the end of December, 1773, and he states, that though Clanssaye stands upon a tertiary deposit, yet it is probable that the gypseous formation of the hills to the eastwards having a westerly dip may pass beneath it: likewise to the earthquakes which affected Kronstadt in Transylvania, Odessa, Bucharest, Lembourg in Gallicia, and Kieff, with other towns in that part of Russia, early in 1838, and in the vicinity of which gypsum is believed to exist. Among the limestone tracts, in which caverns abound, and earthquakes are not unfrequently felt, M. Necker enumerates Fiume, Buchari, Trieste, Lissa in the Adriatic, and Foligno.

In the above instances M. Necker supposes, that cavities having been formed by the action of bodies of water, the roof gave way, and, falling upon a solid floor, produced in the strata a motion which extended laterally and vertically, and gave rise to the phenomenon of an earthquake. He is further of opinion, that air confined in the caverns being also set in motion by the subsidence of the roof, would cause undulations in the overlying strata. To illustrate his views, M. Necker described the vibrations produced in the walls of a house which he occasionally inhabits at Geneva, by the blows of a blacksmith's hammer upon an anvil placed in a vault, and these vibrations always appeared to him completely analogous to the motion which he experienced in the same room during the earthquake on the 19th of February, 1812. He likewise stated, that M. Virlet perceived, in a coal-mine, a shock resembling that of an earthquake, by the falling in of some works at the distance of a quarter of a league.

With respect to the shocks felt at Nice, the author says, that he had carefully compared the list published by M. Risso, with the accounts of eruptions of Vesuvius and Etna; and that though some of the earthquakes had preceded, by very short intervals, certain powerful eruptions of those volcanoes; yet, in very many instances, the shocks appear to have been quite independent; and that a considerable number of eruptions, both of Vesuvius and Etna, had not been felt at Nice. Hence, he infers, that, in this case, there may have been earthquakes due to volcanic, as well as non-volcanic, agents; and that Nice, standing upon a gypsum formation, may have felt the effects of volcanic eruptions in consequence of a predisposition in the undermined ground, without which they would not have been perceptible at the surface.

M. Necker objects to the earthquake in Calabria, in 1783, being considered of true volcanic origin, because it was unaccompanied by any disengagement of heat, lava, smoke, acid, or sulphureous products; because the surface of the ground was depressed, not elevated; because only sand and water were ejected through the fissures and circular or star-like cavities formed in the ground, and because there was no eruption of Vesuvius or Etna. The earthquakes in the valley of the Mississippi, during 1812, he conceives were non-volcanic, in consequence of no lava having been poured forth, nor any acid or other vapours emitted. He alluded to a letter by Mr. Stanley Griswold, dated Kaskahia, Illinois, the 22nd of Dec. 1812, which describes some of the phenomena of the earthquakes,—particularly the subterranean noises resembling thunder, the cracks formed in the ground, the issuing of " a something" like smoke, or warm aqueous vapour, accompanied by a great quantity of sand, the ejection of carbonised wood, coal, and pumice, a quantity of which is said to have been collected on the Mississippi, the drying up of lakes, and the raising of the bed of the river. To some of these statements M. Necker objects. He conceives that the smoke, or warm aqueous vapour, which is mentioned only from the reports of others, and not decidedly, may have been mistaken for vapour produced by water striking against an immoveable obstacle. The occurrence of pumice, he conceives, is very doubtful; and, as it is mentioned by no other author, he withholds his assent till the substance has been examined by a competent mineralogist.

M. Necker dissents from the Cutch earthquake in June, 1819, being considered volcanic. The elevation of the Ullah Bund, he conceives, was effected by the subsidence of the ground towards Sindree, or to a movement on a fixed axis. The materials thrown out by the shocks were only black mud, sand, wrought iron, and nails, and could not therefore, he says, have been produced from any great depth.

The earthquakes on the coast of Cumana, and the Caraccas, M. Necker considers to be non-volcanic; and that when the number and violence of the shocks felt in that part of America are considered, he is of opinion, that the agreement of the earthquakes, in April, 1812, with the simultaneous eruption of the volcano of St. Vincent, was purely fortuitous.

In 1772, the little group, situated some leagues to the north of the chain of the Caucasus, and composed of the trachytic mountains called Pechstein, and the calcareous hill Metschuka, was shaken by an earthquake. The warm springs, known by the name of the baths of the Caucasus, issue from the foot of the limestone hill, and deposit, as well as all the cold brooks, considerable quantities of calcareous tuff. It might be supposed, observes M. Necker, that the thermal springs indicate the existence of some portion of the original heat of the trachyte; and that the earthquake of 1772, by which a portion of the hill, Metschuka, was engulphed, was only the effect of volcanic activity. This, he says, is possible; but it appears to him much more probable, that the cold and warm springs

had formed large cavities in the limestone hill, the falling in of the roof of which produced the shock and attending phenomena.

The earthquakes in Jamaica in 1692, M. Necker is of opinion were non-volcanic, because there were only subsidences of the ground, and because only water, sand, and gravel were ejected.

The earthquake in the plain of Bogota, 16th November, 1827, he is tempted to consider non-volcanic, the country being gypsiferous and saliferous; but he admits that it may have been of a mixed nature, in consequence of the great adjacent volcano of Popayan being, at the same time, in activity. The earthquakes on the coast of Chili, he is of opinion, may have a similar origin.

M. Necker gives a list of earthquakes extracted from Mr. Lyell's "Principles of Geology," and arranges them under the heads—volcanic, non-volcanic, and of doubtful origin.

In the first list he includes the earthquakes felt at Ischia, February 2nd, 1828; Java, 1699, 1772, and 1786; Sumbana, April, 1815; Quito, Feb. 4, 1797; Sicily, March, 1693, 1790; Guatimala, 1773; Kamtschatka, 1737; Peru, Oct. 28, 1746; Iceland, 1725; Teneriffe, May 5, 1706; Sorea, (Moluccas) 1693; Lisbon, Nov. 1, 1755.

Non-Volcanic.—Murcia, 1829; Lahore, Sept. 1827; Lissa, in the Adriatic, 1833; Foligno, Jan. 15, 1832; Cutch, June 16, 1819; Cumana, Dec. 14, 1797; the Caraccas, March 26, 1790; Calabria, 1783 to 1786; Bechstan, 1772; and Jamaica, 1692.

Doubtful Origin.—Bogota, Nov. 16, 1827; Chili; Quebec, Dec. 1791; Nipon, Japan, August 1, 1783; and Martinique, 1772.

Thus, though M. Necker reduces considerably the power of volcanic agents, yet he is far from denying that a weak volcanic movement may be propagated over considerable surfaces; and he mentions, in conclusion, the following instances, as not generally known, of probable connexions between earthquakes and volcanic eruptions. The great eruption of Vesuvius, which commenced the 21st of February, 1822, was preceded by an earthquake at Geneva, and in the province of Bugey, in France, on the 19th of February; and, before the eruption of October of the same year, the environs of Aleppo, in Syria, had been convulsed during the whole of August; the most violent shocks having taken place the 13th of the same month; and on the 14th of August an earthquake was experienced at Laybach in Carniola. On the 19th of February, 1825, the town of St. Maure, in the Ionian Islands, was almost destroyed by an earthquake, felt also at Corfu and Prevesa. During the night of the 20th and 21st of February, 1825, there were several shocks at San Veit in Carinthia; and on the 21st of February, and for five days after, dreadful earthquakes were felt at Alger and its environs. The 25th of February, 1828, Vesuvius, which had been very quiet from 1822, commenced a new eruption. There were earthquakes at Trieste during the night of the 13th and 14th January, 1828, at the Island of Ischia on the 2nd of February, and all over Belgium the 23rd of the same month. Lastly, M. Necker deems it not improbable, that the earthquakes felt in Hungary, Transylvania, Gallicia,

Wallachia, and the south of Russia, at the commencement of 1838, were the precursors of the eruptions of Vesuvius and Etna during the summer of the same year.

ANNUAL GENERAL MEETING, *Feb.* 15, 1839.

The President announced that the Wollaston Medal and £20 had been awarded by the Council to Professor Ehrenberg of Berlin, for his discoveries respecting Fossil Infusoria; and in delivering the Medal with the accompanying sum of money into the hands of the Chevalier Bunsen, who was present, Mr. Whewell addressed him as follows:

Mr. Bunsen,

I have great pleasure in delivering into your hands the Wollaston Medal, which the Council of this Society have awarded to your countryman Professor Ehrenberg, for his discoveries respecting Fossil Infusoria. These discoveries, eminently striking and curious to all intelligent persons, are full of the most lively interest for Geologists. Such discoveries are a just reward of M. Ehrenberg's merits, since he had prepared himself for this success by a profound study of natural history, by persevering and scrutinizing researches, and by extensive and enterprising travels. We gladly give this medal as a proof that we sympathize in the admiration which these discoveries have excited throughout scientific Europe.

To many others, and to myself in particular, there is an additional source of pleasure at having such a communication to make to M. Ehrenberg, in the circumstance of our having recently become acquainted with him, and having seen personally in our own country the evidences of his talents and genius, his simple and strenuous love of knowledge. We beg you to communicate to him with this medal the expression of our admiration in his labours, our deep interest in their results, and our warm wishes that he may long have granted him the health and energy and opportunity which their successful prosecution demands.

Allow me to say also, that we trust that this token of our respect will be kindly received by M. Ehrenberg's countrymen as well as by himself, and that they will accept it as a testimony how gladly we do honour to the profound knowledge and patient research which distinguish that great branch of the European family. I rejoice to be able to deliver this medal into the hands of a distinguished countryman of Professor Ehrenberg; and I cannot but add, as an additional ground of satisfaction, into the hands of one, who, by his wide acquaintance with men of science and learning, and with their works, is so well prepared to sympathise with their honours and successes, as he is by his nature prompted to rejoice in excellence of every kind.

The Chevalier Bunsen acknowledged the distinction conferred upon Professor Ehrenberg in the following terms:—

Sir,—I feel highly gratified by the honour conferred upon me, of receiving at your hands the valued acknowledgement of the merits of my distinguished countryman, Professor Ehrenberg, and I beg to

return thanks, not only in my name, but also in that of Baron Bulow, as the representative of Prussia in this country, who is prevented by official business from being present on this occasion.

Nobody can be more able or inclined to appreciate duly the value of this distinction than Professor Ehrenberg. I know from himself that it was by England in particular that he wished his researches to be examined and approved; and it was especially by this illustrious Society, so worthily presided over by one whose name is also in Germany equally dear to the friends of religion and moral philosophy, and to the followers of the exact sciences: it was to this Society, I say, to whose tribunal he was desirous to submit the judgement of the merits and importance of his discovery. Indeed, the honour you have decreed him to-day is only the public confirmation and solemn badge of that kind and encouraging interest which he met with from the members of this Society, and for which he felt the most sincere gratitude.

But this feeling, Sir, will not be confined to himself: the honour of the prize awarded to him this day amongst so many illustrious competitors of all nations, will be deeply felt by the whole literary public of Germany: it will, I trust, form a new link in that intellectual union between the two great and enlightened nations, which have so many ties of common interest, and so many objects of warm and deep sympathy; an union which must become every day more and more intimate, and prove productive of the most beneficial consequences, not only for the progress of science in the whole range of human intellect, but for the welfare of humanity at large.

The flattering manner in which you have been pleased to allude to myself obliges me to say a few words on my own behalf. I feel only too much how entirely I must attribute those expressions to the kindness that inspired them, knowing how inadequate my own merits are to deserve them. But I rejoice sincerely at having this opportunity offered to me, publicly to express my feelings of gratitude for the kind and generous reception I have constantly met with in this country, which for so many years and for so many and good reasons, has been the object of my love and of my admiration—feelings which will ever remain engraven on my heart, and with a particularly gratifying reference to this day.

The following gentlemen were elected the Officers and Council for the ensuing year:

President.—Rev. W. Buckland, D.D., Professor of Geology and Mineralogy in the University of Oxford. *Vice-Presidents.*—G. B. Greenough, Esq. F.R.S. & L.S.; Leonard Horner, Esq. F.R.S. L. & E.; Charles Lyell, jun. Esq. F.R.S. & L.S.; Rev. Adam Sedgwick, F.R.S. & L.S., Woodwardian Professor in the University of Cambridge. *Secretaries.*—Charles Darwin, Esq. F.R.S.; William John Hamilton, Esq. *Foreign Secretary.*—H. T. De la Beche, Esq. F.R.S. & L.S. *Treasurer.*—John Taylor, Esq. F.R.S. *Council.*—Professor Daubeny, M.D. F.R.S. & L.S.; Sir P. Grey Egerton, Bart. M.P. F.R.S.; W. H. Fitton, M.D. F.R.S. & L.S.; Prof. Grant, M.D. F.R.S.; Rev. Prof. Henslow, F.L.S.; W. Hopkins, Esq.

M.A. F.R.S.; Robert Hutton, Esq. M.P. M.R.I.A.; Sir Charles Lemon, Bart. M.P. F.R.S.; Prof. Miller, M.A.; R. I. Murchison, Esq. F.R.S. & L.S.; Richard Owen, Esq. F.R.S.; Sir Woodbine Parish, K.C.H. F.R.S.; George Rennie, Esq. F.R.S.; Rev. Prof. Whewell, F.R.S.

Address delivered by the Rev. William Whewell, B.D. F.R.S. *President.*

Gentlemen,

The Reports which have been read show that the Society is still in a state of progression as to numbers, although in consequence of some oversights in preceding periods, the comparison of this year's statement with that of last year does not at first sight give an accurate view of our progress.

I venture also to speak of our pecuniary condition as prosperous, although, in the Estimates for the present year the expenses exceed the income. This excess admits of explanation: the estimated expenses include the cost of publishing a Part of our Transactions, and as this occurs only about once in two years, the whole expense ought not to be considered as belonging to one year. Stoves and other articles of furniture, expenses not likely to recur, have also inflamed the debtor side of our account.

There is one considerable article in our estimated expenses, of which payment may not be required, but from which I confess I should be sorry to see the Society liberated. I speak of the salary of our Curator. In my address last year I stated that the Council had it in contemplation to make some arrangement by which Mr. Lonsdale's labours, then far too heavy, should be lightened. This was done, I believe to the satisfaction of every one, by separating the office of Curator from that of Assistant Secretary, and to the former office Mr. Wood was appointed, with a salary of 125*l.* The Council found in Mr. Wood's zeal and knowledge every reason to congratulate themselves on the possession of such an officer; and have heard with regret that the state of his health compels him to resign his office. I trust, however, that the Council will be able to provide some means of rendering the Society's Collection useful, without allowing Mr. Lonsdale to be again burthened with a complication of duties injurious to him and inconvenient to the Society.

Although, as I have said, I look without any inquietude upon the state of our funds, it is impossible not to allow that such an aspect of them makes it necessary to attend to economy wherever it is possible. There is one part of our establishment to which I am compelled, most reluctantly, to apply this remark; I mean, our Library and Museum. I fear that we must consider ourselves as under the necessity of confining within very narrow limits any assistance which can be rendered to those departments from our general funds. And yet we cannot look at these parts of our establishment, and especially at the Library, without seeing that they do in fact require very material additions. Our Library, which ought to possess all the best books and maps which bear upon our science, is desti-

tute of many of them, especially of the more modern works, to an extent which we should hardly any of us find tolerable in our private libraries. This deficiency interferes materially with the utility of the Society, and is indeed inconsistent with its character. We shall, I trust, all agree that it is a state of things we ought to remedy. At no period of the history of this body has there been found wanting, when the occasion demanded it, a liberal and generous spirit among its members; and I am fully persuaded that at the present day the love of the Society has not waxed cold among the Fellows, nor have their purse-strings become rigid. It has appeared to me, that when a definite list of our deficiencies is laid before you, it will not be found difficult for each person to find in such a list some article, book or map, which it will gratify him that the Society should possess as his gift. In this or in some other way I do not doubt that we shall be able to bring up the condition of our Library to that which the time and our position require.

The Council have adjudged the Wollaston medal for the present year to Professor Ehrenberg, for his discoveries respecting fossil Infusoria and other microscopic objects contained in the materials of the earth's strata. We all recollect the astonishment with which, nearly three years ago, we received the assertion, that large masses of rock, and even whole strata, are composed of the remains of microscopic animals. This assertion, made at that time by Professor Ehrenberg, has now not only been fully confirmed and very greatly extended by him, but it has assumed the character of one of the most important and striking geological truths which have been brought to light in our time: for the connection of the present state of the earth with its condition at former periods of its history, a problem now always present to the mind of the philosophical geologist, receives new and unexpected illustration from these researches. Of about eighty species of fossil Infusoria which have been discovered in various strata, almost the half are species which still exist in the waters: and thus these forms of life, so long overlooked as invisible specks of brute matter, have a constancy and durability through the revolutions of the earth's surface which is denied to animals of a more conspicuous size and organization. Again, we are so accustomed to receive new confirmations of our well-established geological doctrines, that the occurrence of such an event produces in us little surprise; but if this were not so, we could not avoid being struck with one feature of Prof. Ehrenberg's discoveries;—that while the microscopic contents of the more recent strata are all freshwater Infusoria, those of the chalk are bodies (*Peridinium*, *Xanthidium*, *Fucoides*,) which must, or at least can, live in the waters of the ocean. Nor has Prof. Ehrenberg been content with examining the rocks in which these objects occur. During the last two years he has been pursuing a highly interesting series of researches with the view of ascertaining in what manner these vast masses of minute animals can have been accumulated. And the result of his inquiries is*, that these creatures exist at present in

* Abhandl. Kön. Ak. Wissensch. Berlin. 1838.

such abundance, under favourable circumstances, that the difficulty disappears. In the Public Garden at Berlin he found that workmen were employed for several days in removing in wheelbarrows masses which consisted entirely of fossil Infusoria. He produced from the living animals, in masses so large as to be expressed in pounds, tripoli and polishing slate similar to the rocks from which he had originally obtained the remains of such animals; and he declares that a small rise in the price of tripoli would make it worth while to manufacture it from the living animals as an article of commerce. These results are only curious; but his speculations, founded upon these and similar facts, with respect to the formation of such rocks, for example, polishing slate, the siliceous paste called *keiselguhr*, and the layers of flint in chalk, are replete with geological instruction.

As the discoveries of Prof. Ehrenberg are thus full of interest for the geological speculator, so have they been the result, not of any fortunate chance, but of great attainments, knowledge, and labour. The author of them had made that most obscure and difficult portion of natural history, the infusorial animals, his study for many years; had travelled to the shores of the Mediterranean and the Red Sea in order to observe them; and had published (in conjunction with Prof. Müller) a work far eclipsing anything which had previously appeared upon the subject. It was in consequence of his being thus prepared, that when his attention was called to the subject of fossil Infusoria, (which was done in June, 1836, by M. Fischer) he was able to produce, not loose analogies and insecure conjectures, but a clear determination of many species, many of them already familiar to him, although hardly ever seen perhaps by any other eye. The animals (for he has proved them to be animals, and not, as others had deemed them, plants) consist, in the greater number of examples, of a staff-like siliceous case, with a number of transverse markings; and these cases appear in many instances to make up vast masses by mere accumulation without any change. Whole rocks are composed of these minute cuirasses of crystal heaped together. Prof. Ehrenberg himself has examined the microscopic products of fifteen localities, and is still employed in extending his researches; and we already see researches of the same kind undertaken by others, to such an extent, as to show us that this new path of investigation will exercise a powerful influence upon the pursuits of geologists. We are sure therefore that we have acted in a manner suitable to the wishes of the honoured Donor of the medal, and to the interests of the science which we all in common seek to promote, in assigning the Wollaston medal to Prof. Ehrenberg for these discoveries.

Although it is not necessary as a ground for this adjudication, it is only justice to Prof. Ehrenberg to remark, that his services to geology are not confined to the researches which I have mentioned. His observations, made in the Red Sea, upon the growth of corals, are of great value and interest; and he was one of the distinguished band of scientific explorers who accompanied Baron von Humboldt in his expedition to the Ural Mountains. And I may further add, that even since the Council adjudged this medal, Prof. Ehrenberg

has announced to the Royal Academy of Sciences of Berlin new discoveries; particularly his observations on the organic structure of chalk; on the freshwater Infusoria found near Newcastle and Edinburgh, and on the marine animalcules observed near Dublin and Gravesend; and, what cannot but give rise to curious reflections, an account of *meteoric paper* which fell from the sky in Courland in 1686, and was found to be composed of Confervæ and Infusoria.

I now proceed to notice some of the most conspicuous names, both among our own countrymen and foreigners, which have been removed by death from our lists since last year.

In Sir Abraham Hume the Society has lost a member who was at all times one of its most strenuous friends and most liberal supporters, and especially in its earliest periods, when such aid was of most value. Indeed he may in a peculiar manner be considered as one of the Founders of the Society. English geology, as is well known, evolved itself out of the cultivation of mineralogy,—a study which was in no small degree promoted, at one time, by the fame of the mineralogical collections of Sir Abraham Hume and others. The Count de Bournon, exiled by the French revolution in 1790, brought to England new and striking views of crystallography, resembling those which Haüy was unfolding in France; and was employed to arrange and describe the mineralogical collections of Sir John St. Aubyn and Mr. Greville, and especially the collection of diamonds of Sir Abraham Hume, of which a description, illustrated with plates, was published in 1816. Some years before this period a few lovers of mineralogy met at stated times at the house of Dr. Babington, whose influence in preparing the way for the formation of this Society was mentioned with just acknowledgement in the President's Address, in 1834, by Mr. Greenough; and certainly he, more fitly perhaps than any other person, could speak of the merits and services of his fellow-labourers. Of the number of these Sir Abraham Hume was one; although not, I believe, one of those who showed their zeal for the pursuits which associated them by holding their meetings at the hour of seven in the morning, the only time of the day which Dr. Babington's professional engagements allowed him to devote to social enjoyments of this nature.

Out of the meetings to which I refer this Society more immediately sprung. The connection of mineralogy with geology is somewhat of the nature of that of the nurse with the healthy child born to rank and fortune. The foster-mother, without being even connected by any close natural relationship with her charge, supplies it nutriment in its earliest years, and supports it in its first infantine steps; but is destined, it may be, to be afterwards left in comparative obscurity by the growth and progress of her vigorous nursling. Yet though geology now seeks more various and savoury food from other quarters, she can never cease to look back with regard and gratitude to the lap in which she first sat, and the hands that supplied her early wants. And our warm acknowledgments must on all due occasions be paid to those who zealously cultivated mineralogy, when geology, as we now understand the term, hardly existed; and who, when the nobler and more expansive science

came before them, freely and gladly transferred to that their zeal and their munificence.

The spirit which prevailed in the infancy of this Society, and to which the Society owed its permanent existence, was one which did not shrink from difficulties and sacrifices; and among the persons who were animated by this spirit Sir Abraham Hume was eminent; his purse and his exertions being always at the service of the body. He gave his labours also to the Society by taking the office of Vice-President, which he discharged with diligence from 1809 to 1813. He died in March last at the great age of ninety, being then the oldest person both in this and in the Royal Society.

Mr. Benjamin Bevan was a civil engineer, and throughout his life showed a great love of science, and considerable power of promoting its purposes. He instituted various researches, theoretical and practical, on the strength of materials*; and it was he who first proved by experiment the curious proposition, that the Modulus of Elasticity of water and of ice is the same. In 1821 he wrote a letter to the secretary of this Society, recommending that the form of the surface of this country should be determined by barometrical measurements of the heights of a great number of points in it,—the barometer which was to be used as a standard being kept in London. Mr. Bevan and Mr. Webster were commissioned to procure a barometer, and Dr. Wollaston recommended one of Carey's barometers, but it does not appear that any further steps were taken. I may remark that recent researches have further confirmed the wisdom of Mr. Bevan's suggestion, that heights should be measured, as all other measurements are made, from some fixed *conventional* standard, instead of incurring the vagueness and inconsistency which result from assuming the existence of a natural standard, such as the level of the sea.

Nathaniel John Winch was born at Hampton Court in the year 1769, and after a voyage into the Mediterranean, and travels in various countries in Europe, settled at Newcastle-upon-Tyne as a merchant. He had early paid great attention to botany, which he continued to cultivate during a long life, and kept up a correspondence with all the leading botanists in Europe. He was one of the earliest, and always one of the most active members of the Literary and Philosophical Society of Newcastle; and, in conjunction with a few of his friends, gave to that town a scientific and cultured character, which still distinguishes it. He was one of the honorary members of this Society; and contributed to its meetings, in 1814, "Observations on the Geology of Northumberland and Durham," and in 1816, "Observations on the Eastern Part of Yorkshire,"†

* To Mr. Bevan our Journal is indebted for many valuable communications.—Ed.

† Besides these papers, Mr. Winch published: "The Botanist's Guide through the Counties of Northumberland and Durham. By N. J. Winch, J. Thornhill, and R. Waugh." 2 vols. 1805.—"Flora of Northumberland and Durham." In the Transactions of the Newcastle Natural History Society, vol. 2.—"An Essay on the Geographical Distribution of Plants through the Counties of Northumberland, Durham, and Cumberland."

which were printed in the fourth and fifth volumes of our Transactions. In these he stated his object to be to combine with his own observations much interesting information on the subjects of the quarries, and coal and lead mines, of those districts, which had long been accumulating, and was widely diffused among the professional conductors of the mines. And these memoirs, though not containing much of originality in their views and researches, were, at the time, of considerable utility. He died May 5th, 1838, and, by his will, left to this Society a very considerable and valuable mineralogical collection, now in our Museum.

Mr. William Salmond, of York, was one of the persons who was most zealously and actively engaged in the examination of the celebrated Kirkdale Cavern. He measured and explored new branches of the cave in addition to those first opened, and made large collections of the teeth and bones, from which he sent specimens to the Royal Institution of London, and to Cuvier at Paris. The bulk of his collection was deposited in the Philosophical Society at York, then newly established.

I now proceed to notice our deceased Foreign Members.

François-Dominique de Reynaud, Comte de Montlosier, was born at Clermont in Auvergne, April the 16th, 1755, the year of the celebrated earthquake of Lisbon. He was the youngest of twelve children of a family of the smaller nobility of that province, and was remarkable at an early age for the zeal with which he pursued various branches of science and literature.

Count Montlosier must ever be considered as one of the most striking writers in that great controversy respecting the origin of basaltic rocks, which occupied the attention of mineralogists during the latter half of the last century; and to which, in so large a degree, the progress and present state of geology are to be ascribed. The theory of the extinct volcanos of Auvergne, the subject of his researches, was the speculation which gave the main impulse to scientific curiosity on this point. It is true that he was not the originator of the opinions which he so ably expounded. Guettard, in 1751, had seen, vaguely and imperfectly, that which it now appears so impossible not to see, the evidences of igneous origin in the rocks of that district: and the elder Desmarest, whose examination of them began in 1763, had made that classification of them, which is the basis, and indeed the main substance, of the views still entertained with regard to the structure of that most instructive region. His map of the district, published in 1774 (in the Transactions of the Academy of Paris *for* 1771, according to a bad habit of that body still prevailing), exhibits the distinction of modern currents of lava, ancient currents, and rocks fused in the places where they now are, which distinction supplies a key to the most extraordinary phænomena, while it reveals to us a history more wonderful still. But striking and persuasive as this view was, and fitted, apparently, to carry

First edition, 1820; second edition, 1825.—"Contributions to the Flora of Cumberland." 1833.—"Addenda to the Flora of Northumberland and Durham." 1836.

with it universal conviction, the theory which it implied, collected, as it seemed at the time, from one or two obscure spots in Europe, was for a while resisted and almost borne down by the opposite doctrine of the aqueous origin of basalt; which came from the school of Freyberg, recommended by the power of a connected and comprehensive system,—a power in science so mighty for good and for evil. Montlosier's Essay on the Volcanos of Auvergne, which appeared first in 1788, was, however, not written with any direct reference to this controversy, but was rather the exposition of the clear and lively views of an acute and sagacious man, writing from the fullness of a perfect acquaintance with the country which he described, in which, indeed, his own estate and abode lay. In its main scheme, although Desmarest's is mentioned with just praise*, the object of this Essay is to criticise and correct a work of M. Le Grand d'Aussy, entitled *Voyage en Auvergne.* But as the main additions to sound theory which this work contains, (a point which here concerns us far more than its occasion and temporary effect) we may, I think, note the mode in which he traces in detail the effects which the more recent currents of lava (those which follow the causes of the existing valleys) must have produced upon the courses of rivers and the position of lakes; and the idea, at that time a very bold and, I believe, a novel one, that lofty insulated ridges and pinnacles of basalt, which tower over the valleys, have been cut into their present form by the long-continued action of fluviatile waters, aided by a configuration of the surface very different from the present. The striking and vivid pictures which Montlosier draws of such occurrences, are to the present day singularly instructing and convincing to those who look at that region with the geologist's eye. After publishing this essay, M. Montlosier, a man of varied and commanding talents, became involved in the political struggles of his time, and was an active member of the National Assembly, to which he was sent as Deputy of the Noblesse of Auvergne. In his place there he resisted in vain the proposals for the spoliation of the clergy; and one speech of his on this subject was very celebrated. After witnessing some of the changes which his unhappy country had then to suffer, he became an exile, and resided in London, where for some years he was the editor of the *Courier Français*, a royalist journal. Under the empire, he returned to France, and was employed in the Foreign Office of the Ministry, but recovered little of his property except a portion of a mountain, which was too ungrateful a soil to find another purchaser. The situation however could not but be congenial to his geological feelings; for his habitation was in the extinct crater of the Puys de Vaches. The traveller, in approaching the door of the philosopher of Randane, had to wade through scoriæ and ashes; and from the deep basin in which his house stood, a torrent of lava, still rugged and covered with cinders, has poured down the valley, and at the distance

* After mentioning Guettard, he says, "Les mémoires de M. Desmarest, publiés quelques années après, entrainèrent tout-à-fait l'opinion publique." (p. 20.)

of a league, has formed a dike and barred up the waters which form the lake of Aidat;—a spot celebrated by Sidonius Apollinaris, Bishop of Clermont in the fifth century, as the seat of his own beautiful residence, under the name of Avitacus. It is curious to remark that Sidonius does not overlook the resemblance between his own mountain and Vesuvius:

> " Æmula Baiano tolluntur culmina cono,
> Parque cothurnato vertice fulget apex."

In this most appropriate abode M. de Montlosier was, in his old age, visited at different times by several distinguished English geologists, some of whom are now present; and invariably delighted them with his unfading interest in the geology of his own region, his hospitable reception, and I may add, his lofty and vigorous presence, according well with his frank and chivalrous demeanour. His ardour of character had shown itself in early age: "From my first youth," thus his Essay opens, "I occupied myself with the natural history of my province, in spite of repulse and ridicule." The same spirit involved him in other struggles to the end of his life; and, indeed, we may almost say, beyond it. He took a prominent part in the political controversies of his day; and few works on such subjects, which appeared in France in modern times, produced a greater fermentation than his "Mémoire à consulter" on the subject of the Jesuits. In this work he maintained that the position of the Jesuits in France was dangerous and illegal; and he must be considered as the originator of that movement in consequence of which their body was, a few years later, suppressed by the government. The expression of his opinions respecting the conduct and influence of the clergy of his country was condemned by the ecclesiastical authorities, and was deemed by them of a nature to exclude him from that recognition of his being a son of the Catholic church, which is implied by the performance of the funeral rite according to its ordinances. This, however, did not prevent the inhabitants of the neighbourhood and the military stationed at Clermont from showing the regard which his intercourse with them had inspired, by attending his sepulture in great numbers. He was buried in a spot previously selected by himself, in the crater of the extinct volcano in which his abode was, in the middle of the scenes which he had from his earliest years loved and studied, and taught others to feel a deep interest in. He died at the age of 83, on his way to Paris in order to take his seat in the Chamber of Peers, of which he was a member*.

* Besides his "Essay on the Extinct Volcanoes of Auvergne," M. de Montlosier was the author of the following works: "Mémoire à consulter sur un Système Religieux et Politique tendant à renverser la Religion, la Société et le Trône" (1826). "Dénonciation aux Cours Royales rélativement au Système Religieux et Politique signalé dans le Mémoire à consulter," (1826). "Mémoires de M. le Comte de Montlosier sur la Révolution Française, le Consulat, l'Empire, et les principaux Evènements qui ont suivis 1755-1830." Of this work two volumes have appeared, which bring the narrative down to the author's quitting the National Assembly in 1790.

Anselme-Gaëtan Desmarest, honorary member of the Royal Academy of Medicine, and Professor of Zoology at the Royal Veterinary College of Alfort, was the son of Nicolas Desmarest, who has just been mentioned as the predecessor of Montlosier in his theory of the volcanic origin of Auvergne. The son also employed himself upon the same district; and published an enlarged and improved edition of his father's map of Auvergne;—a work which is still spoken of with admiration, for its fidelity and skilful construction, by all who explore that country. But the labours of the younger Desmarest were principally bestowed upon the other parts of natural history. We possess in our Library, extracted from various journals, and presented us by the author, his "Notes on the impressions of marine bodies in the strata of Montmartre," published in 1809; his "Memoir on the Gyrogonite," published in 1810; to which he added, in 1812, the recognition of the analogy of this fossil with the fruit of the Chara, pointed out by his brother-in-law M. Léman; his review of a work by M. Daudebard de Ferussac, on the Fossils of Freshwater Formations, in 1813; his memoir on Two Genera of Fossil Chambered Shells, in 1817; and his "Natural History of the Proper Fossil Crustaceans," published in 1822 along with M. Brongniart's "Natural History of Fossil Trilobites." In the "Dictionaire d'Histoire Naturelle," the article Malocostracés, which contains a complete account and classification of Crustaceans, is by M. Desmarest, with others on the same subject. In this work all the articles on Crustaceans had originally been assigned to Dr. Leach; but when the lamented illness of that distinguished naturalist prevented his finishing this task, it was committed to Desmarest, who carefully studied the labours of his predecessor; and, with most laudable industry and self-denial, made it his business to follow his method as closely as possible. He also published a separate work on Crustaceans in 1825.

Count Kaspar Sternberg was one of those persons, so valuable in every country, who employ the advantages of wealth and rank in the cultivation and encouragement of science. He belonged to a younger branch of one of the best and oldest families in Bohemia; and was closely connected with the persons of most elevated station in that country. He was born the 6th of January, 1761, and received a distinguished education at Prague; not only, as was then common among the Bohemian nobility, through private tutors, but by following the public course of the university. He was created Canon of the Chapter of the metropolitan church at Ratisbon, which, obliging him to receive the lower degree of holy orders, bound him to celibacy. At Ratisbon, then a considerable place, and the seat of the Diet of the German empire, he formed friendships with several eminent persons, and especially with Count Bray (afterwards Bavarian minister at various courts), a man of letters, and a distinguished botanist. Count Sternberg also cultivated botany, and became an active member of the Botanical Society of Ratisbon. During the time that Germany was a prey to the miseries of war, he retired to his hereditary country seat Brzezina, in the circle of Pil-

sen, in the north-western part of Bohemia. Here his attention was early drawn to the coal formation, of which mineral he possessed an extensive estate at Radnitz. He soon formed the intention of publishing representations of the fossil vegetables belonging to the coal strata. These had already begun to excite the attention of geologists. Some of these works, containing notices on such subjects, preceded the existence of sound geology, as the *Herbarium Diluvianum* of Scheuchzer, the *Sylva Subterranea* of Beutinger, and the *Lapis Diluvii Testis* of Knorr*. At the beginning of the present century, Faujas de St. Fond had published in the Annales du Muséum some impressions of leaves, not indeed belonging to the coal, but to a later formation. These impressions were examined and determined by Count Sternberg, in the Botanical Journal of Ratisbon, in 1803. In the following year appeared the first truly scientific work on this subject, the "*Flora der Vorwelt*" of Schlotheim, in which the great problem which was supposed to demand a solution was, Whether the vegetables of which the traces are thus exhibited belong to existing or to extinct kinds? Count Sternberg was in Paris when he received the work of Schlotheim, and he studied it carefully by the aid of the collections which exist in that metropolis. He published in the Annales du Muséum a notice on the analogies of these plants, but concluded with observing, that a greater mass of facts was requisite; and that, these once collected, the general views which belong to the subject would come out of themselves.

Bearing in mind this remark of his own, when fortune, after the storming of Ratisbon in 1809, set him down in the midst of the great coal formations of Bohemia, he proceeded forthwith to manage the working of his mines, so as to preserve as much as possible the most remarkable impressions of fossils. Combining his own specimens with those found in other places, he began to publish, in 1820, his "Essay towards a Geognostic-botanical Representation of the Flora of the Pre-existing World." In this work he not only gave a great number of very beautiful coloured engravings of vegetable fossils, but also attempted a systematic classification of them. But he stated, in the first portion of his work†, that the problems, important alike for botany and geology, which offered themselves, could only be solved by combined labours on a common plan; and after mentioning the various European Societies to which he looked for assistance (among which he includes this Society), he adds, "Bohemia and the hereditary states of the Austrian empire, I am ready, with some friends of science, to make the subject of continued investigation." The specimens of which he published representations, with many more, formed the Count's collection at his castle of Brzezina; but he declared in the outset, that as soon as the National Bohemian Museum at Prague was provided with the means of receiving

* To the earlier works on this subject we may add Martin's *Petrificata Derbiensia*, published 1809; and Parkinson's Organic Remains (1804), which contains many plates of vegetables.

† *Erster Heft*, p. 16.

and displaying this collection, the whole should be transferred from Brzezina to the capital. This was afterwards done; and in this and other ways he was one of the principal founders of the Museum at Prague. He also gave notice, that while the collection continued in his own residence, it was open to the inspection of every lover of science, even in the absence of the Count himself.

The publication of Sternberg's *Flora der Vorwelt* went on till 1825, after which it was discontinued till 1838, when two parts appeared, terminating the work. In this last publication he states that he is compelled to give up this undertaking, having been in a great measure deprived of sight for two years, so that he was obliged to devolve the greater part of such labours upon MM. Corda and Presl. His hearing also failed him. He adds, however, that though thus no longer able to pursue the path which he has trodden for twenty years, he shall not fail to render to the science, of which he was one of the founders, any service which may be in his power. This publication was the crowning labour of his life, for he did not long survive it; he retained, however, to the last the elasticity and activity of his mind. He died very suddenly at his country seat already mentioned, on the 20th of December, 1838, being carried off by apoplexy in his 78th year.

In his own country his influence was highly salutary: he directed his attention especially to the improvement of the national education; and we cannot be surprised at finding such a person very soon at the head of nearly all the institutions for literary and public purposes. He founded the National Museum of Bohemia, of which he was the President; gave to it his library and his various collections, and further enriched it at various periods of his life. He was, indeed, zealous in all that concerned Bohemian nationality, and was an accomplished master of the language and literature of his country: since his death I am assured that there is hardly one Bohemian of any class who does not mourn for him as for a most respected benefactor. Throughout Germany, he was looked to by all who felt an interest in science with a respect and regard which he well merited. The emperor Francis held him in the highest esteem; he gave him the title of Privy Councillor, and the Grand Cross of St. Leopold, held in that monarchy as a distinguished honour.

In the preceding sketch I have mentioned Schlotheim as one of the predecessors of Count Sternberg in fossil botany. Although this writer died in 1832, and was an honorary member of this Society, he has never been noticed in the annual address; I may therefore here add a few words with reference to him. Baron E. F. von Schlotheim was Privy Councillor and President of the Chamber at the court of Gotha, and his collection of Petrifactions has long been celebrated throughout Germany. Besides his Flora of a Former World, or Descriptions of remarkable Impressions of Plants, which appeared in 1804, he published, in 1820, 'Petrifactenkunde, or the Science of Petrifactions according to its present condition, illustrated by the Description of a Collection of petrified and fossil remains of the animal and vegetable kingdom of a former world.' And in 1822

and 1823 he published Appendixes to this work. His collection was also further made known by articles in Leonhard's Mineralogical Pocket Book and in the Isis. After his death a new description of this collection was announced, but whether it appeared I am not able to say. Schlotheim's introduction to his account of his collection contains some extensive geological views.

It is only justice to M. de Schlotheim to add here what is said of him by M. Adolphe Brongniart, whose own labours on fossil vegetables have been of such inestimable value to the geologist, and are every year increasing in interest. "Almost half a century," he says, "elapsed, during which no important work appeared on this subject. It was not till 1804 that the 'Flora of the Ancient World,' by M. de Schlotheim, again turned the attention of naturalists to this branch of science. More perfect figures, descriptions given in detail and constructed with the precision of style which belongs to botany, and moreover some attempts at comparison with living vegetables, showed that this part of natural history was susceptible of being treated like the other branches of science: and we may say, that if the author had established a nomenclature for the vegetables which he described, his work would have become the basis of all the succeeding labours on the same subject."

CAMBRIDGE PHILOSOPHICAL SOCIETY.

February 18th.—Dr. Graham the president, in the chair. The conclusion of a paper by Mr. Rothman was read, "on the ancient climate of Italy and other countries." Also, a paper, by Mr. Potter, "on the determination of the length of an undulation of light by various methods;" and an appendix, by Mr. Green, to a former paper on waves.

March 4th.—Dr. Graham in the chair. Various presents were announced, and among the rest, as presents from the Natural History Society of Liverpool, two casts, and several lithographic prints of the footsteps of an unknown animal, found in the sandstone of the promontory of Wirrall, which lies between the mouth of the rivers Mersey and Dee. The most conspicuous of these footsteps agree exactly with those found by Professor Kaup, in Saxony, ascribed by him to an animal which he has termed the Chirotherium. Afterwards Mr. Hopkins gave an account, illustrated by diagrams, of the geology of the parts of England and France in the neighbourhood of the British Channel.

March 18th.—Dr. Graham in the chair. A communication was made by Mr. Earnshaw, on the equilibrium of a system of particles. After this the Astronomer Royal gave an account of the mode now employed for observing the diurnal changes of the variation of the magnetic needle. He also urged the importance of having observations corresponding with the simultaneous observations now made in various parts of Europe, undertaken by some persons interested in the subject residing in Cambridge.

FRIDAY-EVENING MEETINGS AT THE ROYAL INSTITUTION.

February 22nd.—Mr. Johnston on the leading distinctions in the investigation of mental and physical phænomena.

March 1st.—Mr. Brande on steel.

March 8th.—Mr. Brayley on the equilibrium of the atmosphere as dependent on the united action of gravity and temperature.

March 15th—Mr. Cowper on pottery.

March 22nd.—Mr. Faraday on Airy's correction of the ship's compass in an iron vessel.

ROYAL ACADEMY OF SCIENCES OF PARIS.

April 15th.—A paper "On a new Voltaic Combination," by W. R. Grove, Esq., M.A., M.R.I., was communicated by M. Becquerel, for a copy of which, as follows, we are indebted to the author.

Mr. Porrett was, I believe, the first who employed a bladder to separate the liquids in the operating cell of the voltaic pile. M. Becquerel, by introducing this into the exciting cells, has shown us how to render constant the primitive intensity of the battery by preventing cross precipitation*; Mr. Daniell has remedied some practical defects in M. Becquerel's arrangement, and his form of battery is undoubtedly the best of any that have been hitherto proposed†.

In a letter published in the Phil. Mag. for February, (p. 127) I endeavoured to show, that in addition to the immense benefit derived from constancy of action, which was the object aimed at by these gentlemen, the combination of four elements was capable of producing a much more powerful development of electricity than that of three, as by this means we have nearly the sum of chemical affinities instead of their difference; I also there suggested that if the principles I had laid down were true, there was every probability of superior combinations being discovered; I have lately been fortunate enough to hit upon a combination which I have no hesitation in pronouncing much more powerful than any previously known. The experiments which led to it are curious, and possess an interest of their own, as they prove a well-known chemical phænomenon to depend upon electricity, and thus tighten the link which binds these two sciences. The effect to which I allude is the dissolution of gold in nitro-muriatic acid; this metal, as is well known, not being attacked by either of the acids singly. The following experiments leave, I think, no doubt as to the rationale of this phænomenon.

1st. Into the bottom of a wine glass I cemented the bowl of a tobacco pipe; into this was poured pure nitric acid, while muriatic acid was poured into the wine glass to the same level: in this latter acid two strips of gold leaf were allowed to remain for an hour, at the end of which time they remained as bright as when first immersed. A gold wire was now made to touch the nitric acid and the extremity of one of the strips of gold leaf; this was instantly dissolved, while the other strip remained intact.

2nd. The experiment was inverted, but offered some difficulty, as the gold would not remain an equally long time in the nitric acid,

* Ann. de Chimie, vol. xli. M. Becquerel, by employing another form of diaphragm, that of moistened clay, has produced those results of electro-crystallization which are so generally known. [See Scient. Mem. vol. i. p. 414.]

† Phil. Trans. 1836. [See L. and E. Phil. Mag. vol. xii p. 350.]

from the effect of the nitrous gas; enough, however, was ascertained to prove that to the gold in this acid contact made little or no difference; while the gold in the muriatic was always dissolved.

3rd. A platina arc was used for connexion instead of gold: the effect was the same.

4th. The outside of the pipe was coated with gold leaf, leaving scarcely any part exposed: a strip was placed in the muriatic acid as before, and when contact was made with the nitric acid this strip was destroyed, while the coating of gold directly across the line of junction was unhurt.

5th. The nitric acid was stained with a little tournesol: when contact was made, I could not see that the muriatic acid acquired any of the colour.

6th. Nitrate of copper was used instead of nitric acid; the effect was the same, but took place more slowly, and I could detect no precipitation on the negative metal.

7th. I now made gold leaf in muriatic acid the electrodes of a single pair of voltaic metals; the acid was decomposed and the positive electrode was dissolved.

From all this I think we may pronounce the action to be as follows: as soon as the electric current is established, both the acids are decomposed, the hydrogen of the muriatic unites with the oxygen of the nitric, and the chlorine attacks the gold.

In all these cases the currents were examined with a galvanometer, and in all, the gold which was dissolved represented the zinc of an ordinary voltaic combination: the greatest deflection was obtained with platina, gold, and the two acids. It now occurred to me, that as gold, platina, and two acids gave so powerful an electric current, *à fortiori*, the same arrangement, with the substitution of zinc for gold, must form a combination more energetic than any yet known. I delayed not to submit this to experiment, and was gratified with the most complete success. A single pair, composed of a strip of amalgamated zinc an inch long and a quarter of an inch wide, a cylinder of platina three quarters of an inch high, with a tobacco-pipe bowl and an egg cup, readily decomposed water acidulated with sulphuric acid. In this battery the action is constant, and there is no precipitation on either metal: it offers the great advantage of being able to utilize the action of concentrated nitric acid. I tried the same arrangement, substituting for the muriatic acid caustic potass, which was suggested to me by a well-known experiment of M. Becquerel: the action was equally powerful, and I should prefer this arrangement, as there is no necessity for amalgamating the zinc, but for a fatal objection—the nitrate of potass, crystallizing in the pores of the earthenware, splits it to pieces; except, therefore, a new description of diaphragm be discovered which will bear the action of powerful acids, this combination must be abandoned.

I diluted the muriatic acid with twice its volume of water and the effect was not perceptibly inferior. I then tried sulphuric with four or five times its volume of water; the intensity was a little diminished, but so little that I should prefer this combination to any other, as

cheaper, exercising less local action on the zinc, and by no possibility endangering the platina. The nitric acid may be the common acid of commerce, but must be concentrated. If the hydrogen, instead of being absorbed by the oxygen of the nitric acid, is evolved on the surface of the platina, the energy of action is lowered and is no longer constant.

Great advantage will be found in employing a cell divided by a porous diaphragm for a decomposing cell; thus if oxygen gas be wanted, the positive electrode should be put into dilute sulphuric acid and the negative into concentrated nitric. If chlorine be wanted, the positive into muriatic, the negative into nitric; if hydrogen, both into muriatic, the positive one being of amalgamated zinc, &c. &c. By this means, and with a small battery of the description I am about to indicate, a traveller may carry in his pocket an electro-chemical laboratory.

I have constructed a small battery, of a circular shape, consisting of seven liqueur glasses and seven pipe bowls; the diameter is four inches, the height one inch and a quarter: this pocket battery gives about one cubic inch of mixed gases in two minutes. The form of this combination is in effect that of Mr. Daniell, the connexions, however, never need adjustment but when the worn-out zinc is renewed; and in a battery which M. Becquerel and myself are about to construct, I hope to remedy another defect, viz. the necessity of pouring solutions separately into each cell, which is troublesome and injurious from the inequality of strength which results; I have entirely remedied this in a copper and zinc constant battery; but though the process be simple, it would require more words to describe than a matter of such minor importance is worth.

LIX. *Intelligence and Miscellaneous Articles.*

LATANIUM—A NEW METAL.

M. BERZELIUS, in a letter to M. Pelouze, dated the 22nd of February, states that M. Mosander, in submitting the cerite of Bastnaes, in which cerium was met with twenty-five years ago, has discovered a new metal.

The oxide of cerium, separated from the mineral by the usual process, contains nearly two-fifths of its weight of the oxide of the new metal, merely altered by the presence of the cerium, and which, so to speak, is hidden by it. This consideration induced M. Mosander to give the new metal the name of *latane* or *lantan*.

It is prepared by calcining the nitrate of cerium, mixed with nitrate of latanium. The oxide of cerium loses its solubility in weak acids; and the oxide of latanium, which is a very strong base, may be separated by nitric acid, mixed with 100 parts of water.

Oxide of latanium is not reduced by potassium; but by the action of potassium on the chloride of latanium a grey metallic powder is obtained, which oxidizes in water with the evolution of hydrogen gas, and is converted into a white hydrate.

The sulphuret of latanium may be produced by heating the oxide strongly in the vapour of oxide [sulphuret?] of carbon. It is of a pale yellow colour, decomposes water with the evolution of hydrosulphuric acid, and is converted into a hydrate.

The oxide of latanium is of brick-red colour, which does not appear to be owing to the presence of oxide of cerium. It is converted by hot water into a white hydrate, which restores the blue colour of litmus paper reddened by an acid; it is rapidly dissolved even by very dilute acids; and when it is used in excess, it is converted into a subsalt. The salts have an astringent taste, without any mixture of sweetness; the crystals are usually of a rose-red colour. The sulphate of potash does not precipitate them, unless they are mixed with salts of cerium. When digested in a solution of hydrochlorate of ammonia, the oxide of latanium dissolves, with the evolution of ammonia. The atomic weight of latanium is smaller than that assigned to cerium; that is to say, to a mixture of the two metals.

M. Berzelius has repeated and verified the experiments of M. Mosander.—*L'Institut*, 14 *Mars*, 1839.

FALL OF METEORITES AT THE CAPE OF GOOD HOPE*.

"On the morning of the 13th October, about nine o'clock, a fall of stones (of which a specimen is herewith sent) occurred in the Bokkeveld, about fifteen miles from Tulbagh, attended with the most awful noise, louder and more appalling than the strongest artillery, causing the air to vibrate for upwards of eighty miles in every direction. Indeed it was felt from the Cape Flats to the edge of the Great Karroo, and again from Clan William to the river Zonderend, near Swellendam. The noise was awful; and by those in the immediate neighbourhood of the spot where the stones fell, is described as something similar to the discharge of artillery, by those at a greater distance as rocks rolling from a mountain; which was the sensation at Worcester, some forty miles from the chief site of the phænomenon. Many felt a curious sensation, especially about the knees, as if they had been electrified. At the time of the occurrence I was on the very skirts of its influence, on the edge of the Karroo, in company with the Hon. Mr. Justice Menzies. At the moment of the explosion I witnessed a volume of the electric fluid forcing its way from the west in the form of a Congreve rocket; it exploded almost immediately over my head, into apparent globules of fire, or transparent glass. Throughout the region of the phænomenon the air was highly charged with the electric fluid, especially the night prior to the fall of the stones; the mountains around Worcester and the Bokkeveld being in one continued blaze of lightning, and some of the inhabitants described the fire as rising from the earth. The stones (the quantity I have not been able to ascertain, but supposed several cwts.) fell in the presence of a farmer, who had with him a Hottentot, who stood so near the shower as to become perfectly insensible for some time, either from the electricity or from the effects of fright. The stones fell in three spots, but all

* See p. 231, and also p. 368 of the present Number. The specimen mentioned by Mr. Thompson is now in the British Museum.—E. W. B.

within a square of forty or fifty yards. Some fell on hard ground, when they were smashed into small particles; others in soft ground, where they were dug out. Prior to the real cause of the phænomenon being known, it was taken for an earthquake." *Letter from* G. Thompson, *Esq. of Cape Town, dated Nov.* 28, 1838, *inserted in* Charlesworth's *Magazine of Natural History,* vol. iii. p. 145.

"On the 13th of October last (1838) a most brilliant meteoric appearance was seen about seventy miles from Cape Town, attended by a shower of stones which extended itself about 150 miles, that is, some stones have been found at 10, 15, 20, 50, and so on miles, all in the same line of direction. Some say that the stones were so soft at first that they could be cut with a knife, and others say that in falling the ground was torn up to some depth and a loud explosion was heard." *Letter from* E. J. Jerram, *Esq. of Cape Town, dated Jan.* 29, 1839, *addressed to* Mr. Brayley.

SELENIURET OF MERCURY.

Del Rio and Kersten have already described some Mexican ores of mercury which contain selenium, but hitherto they appear to have been found only in very small quantity. Very lately M. Ehrenberg has received some minerals from M. Carl. Ehrenberg, director of the Real del Monte mines, among which there is a series of mercurial ores met with at San Onufre, where they are so abundant, that it is intended to work them on the large scale to extract the mercury. In colour and lustre the mineral resembles fahlerz, and occurs without the least indication of foliated structure, disseminated in a gangue of calcareous and heavy spar, from which it is extremely difficult to separate them in a quantitative analysis. It is entirely volatile; the sublimate, reduced to powder, is black, without any admixture of red.

This mineral consists of sulphuret and seleniuret of mercury; and analysis shows that its composition closely approximates four atoms of the first and one atom of the last. It is probable that these two isomorphous bodies may combine in all proportions.—*L'Institut,* 14 *Mars,* 1839.

NONEXISTENCE OF CARBONATES OF QUINA AND CINCHONIA.

M. Langlois makes the following statements respecting the nonexistence of the above compounds:

Chemical works make no mention of the action of carbonic acid upon organic bases. M. Berzelius (*Chimie,* tome 5,) says, however, that carbonate of cinchonia is obtained by double decomposition from a soluble salt of this alkali, and a solution of carbonate of potash. The decomposition certainly occurs; and a white precipitate falls, which, after being washed, and dried between folds of paper, effervesces slightly on the addition of weak acids, which unquestionably indicates the presence of a carbonate; but this effervescence does not occur unless the carbonate of potash has been employed in excess. Imagining then that this effect might be occasioned by the carbonate of potash retained in the cinchonia, notwithstanding repeated washings, the precipitate was treated with cold concentrated

alcohol, which did not dissolve it entirely. A white powder remained, which was thrown upon a filter, and this was found to be carbonate of potash. The alcoholic solution, exposed to the air, evaporated slowly, and deposited crystallized cinchonia, combined with carbonic acid.

The salts of quina, treated in the same way, yielded similar results; and hence it may be concluded, that carbonates of organic bases do not exist.—*L'Institut,* 28 *Mars,* 1839.

MICA CONTAINING POTASH AND LITHIA.

M. V. Regnault has analysed these micas; they fuse easily at a red heat, and without suffering any sensible loss of weight, and are afterwards easily reduced to a fine powder.

The analysis was performed by acting upon the mica, previously fused and reduced to fine powder, with hydrochloric acid, and separating the silica in the usual way. The alumina and peroxide of iron were precipitated together by carbonate of ammonia; the liquors being evaporated, after the addition of sulphuric acid, left a residue, which, when calcined, yielded the alkaline sulphates, which were dissolved in water, and decomposed by chloride of barium. The excess of barytes added was afterwards precipitated by dilute sulphuric acid, added gradually; and the solution containing the alkaline chlorides, after the addition of chloride of platina, were evaporated nearly to dryness. By the addition of alcohol, the double chloride of potassium and platina was separated; the lithia was determined by difference, and by the composition of the sulphates.

In order to determine the fluorine, the mica was acted upon with carbonate of soda, and then treated with boiling water. The alkaline liquor was concentrated after filtration, and then subjected to a current of carbonic acid gas, which produced an abundant precipitate of glutinous silica. A solution of oxide of zinc in carbonate of ammonia was afterwards added to the filtered liquor, and it was then evaporated to dryness; the last traces of silica and alumina were thus separated. The saline mass was treated with a small quantity of boiling water, and the liquor was supersaturated with hydrochloric acid in a platina capsule. The solution was suffered to remain for twenty-four hours, in order to allow the carbonic acid to separate perfectly. It was then saturated by ammonia, and the fluorine precipitated by chloride of calcium.

ROSE MICA LEPIDOLITE.

This mica has the form of very small rose-coloured plates. It is found disseminated in a kaolin, which is employed in the porcelain manufactures of Vienna. It is separated by washing from the kaolin. The mean of four analyses gave

Silica	52·40
Alumina	26·80
Potash....................	9·14
Lithia	4·85
Fluorine	4·40
Deutoxide of manganese	1·50—99·09.

Yellow Mica.

Silica	49·78
Alumina	19·88
Peroxide of iron	13·22
Potash	8·79
Lithia	4·15
Fluorine	4·24—100·06.

Annales de Chimie et de Physique, pp. 69–72.

METEORIC IRON FROM POTOSI.

H. M. Juben, a Lieutenant in the French Navy, among other minerals which had been presented to him, brought from Peru a piece of meteoric iron found near Potosi in Bolivia; it was stated to him to be meteoric iron of great purity; it is cavernous, filled with vacuities, most of which are irregular, but some have the form of a rhombic dodecahedron; some of them also are filled with a greenish vitreous substance similar to the olivine of Pallas. No traces whatever of fusion appear, although the mineral evidently indicates the action of a high temperature. The tenacity of this iron is extremely great, but it is readily hammered and filed. It does not oxidize even when exposed to a moist atmosphere. Its sp. gr. is 7·736. The mean of three analyses performed by M. Morren give us its composition

Iron..................	90·241
Nickel................	9·759
	100·

This iron is remarkable on account of the large quantity of nickel; no trace either of copper, cobalt, or manganese was discoverable. The specimen is deposited in the Museum of Angers.—*Chronique Scientifique*, 24 *Feb.*, 1839.

PHOSPHORESCENT POWER OF ELECTRICAL LIGHT.

M. Becquerel has read a memoir to the Institute on some new properties which electrical light possesses of acting as a phosphorescent power. After a brief history of what had been previously done with respect to phosphorescence, M. Becquerel proceeds as follows:

I shall begin with first showing that electrical light is capable of producing phosphorescence, not as a consequence of a shock or by electrical influence, as was formerly supposed, but on account of properties peculiar to its radiation. For this purpose some freshly calcined oyster-shells are placed in a porcelain capsule, and at a distance of about $\frac{8}{10}$ of an inch, a discharge from eighteen jars is passed through them. The shells soon became luminous, and the light disappears more or less readily according to their degree of excitability.

On successively placing the shells at a distance of about 4, 20, 80, 130, &c. inches, the phosphorescence always appeared; the effect diminishing in proportion to the distance. It is even apparent at a much greater distance still, and where usual electrical influences are not distinguishable. I will also add that green fluors exhibit the

same appearances when submitted to the action of electric light. Nor is this all; if slightly excitable oyster-shells be placed at a distance of several inches, the phosphorescence produced by the first discharge is usually weak; on the second discharge it is stronger, and on continuing the discharges, the luminous property is still further increased, and acquires a considerable degree of intensity. It is therefore evident, that the direct electric light, acting at a distance, predisposes the particles of the shells more and more to become phosphorescent. I ought not to omit to mention, that in these cases I perceived a smell of sulphuretted hydrogen, derived from the action of the sulphuret of calcium on the water contained in the air, and that it became more sensible as the number of the discharges increased, which seems to indicate that the tendency to decomposition increases with the luminosity.

These first observations being finished, and especially considering the before-mentioned experiment, from which no inference had been drawn, that is to say, that calcined shells inclosed in glass tubes and exposed to electric discharges were only phosphorescent on account of the increase of temperature, it occurred to me to try whether substances traversed by diaphragms would lose or preserve their property of becoming phosphorescent at a distance. The substances which I employed as screens were white glass, red glass coloured by protoxide of copper, violet glass, and also of various other tints, glazed paper and leaves of gelatin. I was perfectly aware, that except red glass, the other coloured glasses would not admit of the passage of simple rays, but I thought that these substances would nevertheless suffice to afford differences sufficiently marked, as to the mode in which electric light, the subject of my inquiry, would act.

The discharge from eighteen jars was passed through recently calcined oyster-shells, contained in a capsule, at a distance of about 0·4 of an inch. The experiment was made in a dark chamber, in which I had remained for a quarter of an hour, in order ro render the retina sensible to feeble light, and my eyes remained closed after the discharge, in order that the sight might not be impaired by the impression of the electric light. The shells immediately appeared strongly illuminated; the experiment was repeated in ten minutes, a plate of glass about 0·0117 of an inch thick being placed upon the capsule. The discharge again produced phosphorescence, but in an infinitely less degree than before the interposition of the screen. On increasing the thickness of the glass to about 0·0315 inch, the phosphorescence was still weaker, although the glass was perfectly diaphanous. This experiment repeated at a distance of about four and even at eight inches, still produced phosphorescence, but in a smaller degree. A plate of glass about 0·04 of an inch thick, also gave very weak phosphorescence, and so also did a sheet of very transparent glazed paper of $\frac{1}{5}$ the thickness of the glass.

We have here, therefore, very diaphanous bodies, which allow of the greater part of the luminous rays to pass, and which take away from these same rays the property by which they render bodies phosphorescent.

A plate of red glass, of 0·08 of an inch thick, entirely deprived the light of phosphorescent power, whereas a similar piece of violet glass acted nearly like colourless glass, though in some cases the effect appeared more strongly marked. Yellowish-green glass entirely deprived electric light which traversed it of its phosphorescent power. It appears therefore that colourless glass deprives luminous rays of the greater part of their phosphorescent power, and that the quantity of this power which is taken away by the violet glass, is gradually increased as blue, yellow, and orange glasses are used, the last mentioned entirely destroying the phosphorescent power.

The following experiment also confirms the effect which has been described with screens of white glass placed in the passage of the light: recently calcined oyster-shells were exposed to the light of a bit of phosphorus burning in a bottle filled with oxygen gas. The light emitted was most intense, and yet the phosphorescence which it occasioned was slightly so.

To conclude, it is evident that electric light, besides chemical and calorific luminous properties, also possesses phosphorescent power, which is taken from it partly or almost totally by different substances, which allow the light to pass through them without any sensible diminution.—*L'Institut*, No. 270.

ON THE PREPARATION OF SELENIC ACID. BY HENRY ROSE.

M. Mitscherlich's process for preparing this acid with selenium or a metallic seleniuret consists, as is well known, in fusing them with nitrate of potash or soda. According to Berzelius, it may be obtained with selenious acid by converting it into selenite of potash, mixing the solution of this salt with a little caustic potash, and passing chlorine gas through the solution to complete saturation: by this process there is obtained a mixture of chloride of potassium and seleniate of potash. These two methods yield selenic acid combined with an alkali, and it is difficult and tedious to separate and combine it with certain other bases.

In his first experiments with chlorine gas upon the metallic seleniurets of the Hartz, the solution obtained by mixing the voltatile chloride of selenium with water, through which excess of chlorine was passed, yielded no precipitate of selenium on the addition of a solution of an alkaline sulphite; and this re-agent produced no precipitate of selenium till hydrochloric acid was added and boiled with it for a long time. The selenium evidently existed in the liquor in the state of selenic acid, which was not reduced to that of selenium by the alkaline sulphites, till the moment at which the hydrochloric acid converted it into selenious acid.

If then it be desired to prepare free selenic acid, the best process is that of passing chlorine gas through a solution of chloride of selenium or selenious acid. It is then obtained, mixed only with hydrochloric acid, which when dilute and cold, does not reduce the selenic acid.

The best method of producing selenic acid immediately from selenium is the following: reduce the selenium to coarse powder, put it

into a moderate sized glass vessel, and cover it to the depth of a few lines with water. Into this mixture chlorine gas is to be slow passed, through a pierced cork in the bottle; the gas tube is to be directed upon the selenium through the stratum of water. The selenium is at first converted into brown liquid chloride of selenium, and then into white solid chloride, before it dissolves in the water.

When the liquid chloride of selenium has been formed, it may remain for a considerable time under the water, if that be suffered to remain undisturbed; but when by agitation they are mixed, the water becomes red, on account of the finely divided selenium; for as is well known, the chloride on mixing with water deposits a portion of its selenium; but the chloride is readily soluble in water by means of chlorine gas.

When the selenium is perfectly dissolved in the small quantity of water, the solution is largely diluted with water, and chlorine gas is again passed into it until it is in excess. The excess of chlorine is afterwards allowed to evaporate in a capsule exposed to the air, or by the application of a very gentle heat; by this there is obtained a solution of selenic acid, which contains hydrochloric acid, but no selenious acid.

1·643 gram of selenium converted by this process into selenic acid, yielded by the addition of a solution of chloride of barium 5·787 gram of seleniate of barytes. By calculation, this quantity ought to yield 5·819 of the barytic salt. The small difference of 0·032 gram, is partly owing to the vaporization of a small portion of the chloride of selenium by the excess of chlorine, and also partly because the seleniate of barytes is not as completely insoluble in an acid solution as the sulphate of barytes.—*Ibid.*

ANALYSIS OF CRYSTALLIZED PERIKLINE. BY M. C. T. THAULON IN H. ROSE'S LABORATORY.

Silica..............	69,00
Alumina	19,43
Soda	11,47
Lime	0,20.

G. Rose found the specific gravity of crystals of perikline from the Tyrol, reduced to a coarse powder, to be from 2,637 to 2,645, and he concludes from the analysis and specific gravity that perikline and albite, or cleavelandite are one and the same mineral *.

Poggendorff's Annalen, vol. 42. p. 571.

CISSAMPELIN, A NEW VEGETABLE COMPOUND.

M. Wiggers states that he has discovered a new vegetable base in the root of the *Cissampelos Pareira*; it is obtained by repeated boiling the root in water containing sulphuric acid, and mixing the decoctions with carbonate of soda; a brownish grey precipitate is

* The forms and measurements are also similar, except that the crystals of cleavelandite are usually twins and those of perikline single ones.—Edit.

thus obtained, which is washed, and then redissolved in water containing sulphuric acid. This solution is to be treated with animal charcoal; carbonate of soda added, precipitated a dirty yellow substance, which is to be dried, pulverized, and frequently digested in æther. By this operation a nearly colourless solution is procured, which by the distillation of the æther leaves the cissampelin; in order to purify this substance completely it is to be dissolved in dilute acetic acid, and the solution when diluted and moderately heated is to be again precipitated by carbonate of soda, and the precipitate carefully washed and dried.

M. Feneulle had previously examined this root, and had noticed the existence of a yellow bitter substance in it, which was probably impure cissampelin.—*Journal de Pharmacie*, Jan. 1839.

ANALYSIS OF CRYSTALLIZED OLIGOCLAS * FROM ARENDAL OR SODA-SPODUMENE. BY ROBERT HAGEN IN THE LABORATORY OF H. ROSE.

Soda	9,37	
Potash	2,19	
Lime	2,44	
Magnesia	0,77	
Alumina	23,09	
Silica	63,81	
		101,37

Poggendorff's Annalen, vol. 44. p. 329.

ACTION OF ACIDS ON IODIDE OF SODIUM. BY JUSTUS LIEBIG.

M. Preuss has described in the 26th vol. of the *Annales de Pharmacie*, a peculiar property of iodide of sodium obtained by dissolving iodine in a solution of soda, evaporation and calcination. The fused mass, after solution in water, yielded crystals, which were more difficultly soluble than iodide of sodium, and the solution immediately gave a precipitate of iodine on the addition of hydrochloric and sulphuric acid; this property is possessed by iodide of sodium only when treated with certain oxacids, as nitric acid, &c., and then in a much smaller degree.

This property is explained by the circumstance, that if iodate of soda be calcined, either with or without excess of alkali, this salt allows iodine to separate in the latter case, and is decomposed into oxygen, and a compound first noticed by M. Magnus. It may be considered as a basic hypoiodite of soda $I^2 O + 2 Na O$; is not decomposed a heat below whiteness, and is by water converted into iodide of sodium and iodate of soda.

The crystals which M. Preuss obtained from the fused mass, are formed of a double compound of iodate of soda and iodide of sodium. By the addition of an acid to the solution, the sodium of the iodide

* We have seen several specimens in this country ticketed oligoclas, not one of which bore any resemblance to soda-spodumene, but were apparently green cleavelandite.—EDIT.

of sodium is oxidized at the expense of the oxygen of the iodic acid, of the iodate of soda; and the iodine of the latter, as well as of the iodide of sodium, separate in the state of a thick precipitate: this observation ought to be attended to in preparing the iodide of sodium. The best plan is to redissolve the mass obtained by dissolving the iodine in liquid soda, and heated to redness, and pass hydrosulphuric acid gas into it, until no more sulphur is precipitated, to separate this by the filter and to crystallize the iodide of sodium.—*Ibid.*

METEOROLOGICAL SOCIETY.

A volume has made its appearance under the title of Transactions of the Meteorological Society. As it includes matter which was not deemed fit for publication by the Council of the Society, some members of that body think that the subject requires explanation, and desire not to be held responsible for its contents.

METEOROLOGICAL OBSERVATIONS FOR MARCH, 1839.

Chiswick.—March 1. Cloudy. 2. Very fine. 3. Foggy: fine. 4. Cold haze. 5. Bleak and cold. 6. Frosty. 7. Sharp frost. 8. Cloudy and cold. 9. Frosty: fine. 10. Frosty: cloudy. 11. Dry haze. 12. Frosty: hazy. 13. Hazy. 14, 15. Rain. 16. Fine. 17. Overcast. 18. Cold haze. 19. Cloudy: frosty at night. 20. Rain. 21. Cloudy: fine: rain. 22. Cloudy. 23, 24. Fine. 25. Overcast. 26. Dry haze. 27, 28. Showery. 29. Fine. 30. Cold dry haze. 31. Overcast: rain.

Boston.—March 1—3. Cloudy. 4. Fine. 5. Cloudy. 6. Cloudy: hail and snow early A.M.: more snow P.M. 7. Cloudy: snow early A.M. 8. Stormy with snow. 9—12. Fine. 13—15. Rain: rain early A.M. 16. Cloudy: rain early A.M. 17. Cloudy. 18. Cloudy: snow A.M. 19, 20. Cloudy. 21. Cloudy: rain A.M. 22—24. Cloudy. 25, 26. Fine. 27. Cloudy: rain early A.M.: rain A.M. 28. Cloudy: rain, hail, and snow with thunder and lightning P.M. 29—31. Fine.

Applegarth Manse, Dumfries-shire.—March 1. Occasional showers A.M.: heavy rain and wind P.M. 2. Fine spring day: little raw frost morning. 3. Clear day: wind rather piercing. 4. Cold and ungenial. 5. Cold: dry A.M.: slight snow P.M. 6. Calm cold day: frost keen. 7. The same: showers of snow P.M.: frost. 8. Cold and bleak: hills white: frost continued. 9. Frost continuing: mod. barometer falling. 10. Still frosty: fine day though cold. 11. Snow two inches deep: frost giving way. 12. Snow gone: very chill and slight frost. 13. Temperate: wet afternoon. 14. Damp day: rain in the evening. 15. Calm moist day: drizzling P.M. 16. Spring day, though somewhat raw: rain P.M. 17. Cold and stormy: hills white: frost P.M. 18. Quiet day: frost gone: drizzling P.M. 19. Frosty morning: moderate: cloudy P.M. 20. Moist all day: rain heavy P.M. 21. Mild spring day: occasional slight showers: wind. 22. Boisterous morning, with severe snow showers. 23. Unsettled weather: slight showers, with wind. 24. Still very changeable: occasional showers. 25. Showery: unsettled: snow on the hills. 26. Hoar-frost morning: ice a quarter of an inch thick: rain P.M. 27. Heavy rain A.M.: cleared up: rain again P.M. 28. Rainy morning: cleared up and was fine. 29. Cold drying day: threatening frost P.M. 30. Very cold and dry: cloudy P.M. 31. Cold: threatening rain came on P.M.

Sun, 25 days.	Wind westerly, 2 days.
Rain, 15 days.	Calm, 9 days.
Frost, 10 days.	Moderate, 9 days.
Snow, 6 days.	Brisk, 8 days.
Wind southerly, 13 days.	Strong breeze, 3 days.
—— easterly, 9 days.	Stormy, 2 days.
—— northerly, 7 days.	

Meteorological Observations made at the Apartments of the Royal Society by the Assistant Secretary, Mr. Roberton; *by* Mr. Thompson *at the Garden of the Horticultural Society at Chiswick, near London; by* Mr. Veall *at Boston, and by* Mr. Dunbar *at Applegarth Manse, Dumfries-shire.*

Days of Month. 1839. March.	Barometer. London: Roy. Soc. 9 a.m.	Barometer. Chiswick. Max.	Barometer. Chiswick. Min.	Barometer. Boston. 8¾ a.m.	Barometer. Dumfries-shire. 9 a.m.	Barometer. Dumfries-shire. 8¾ p.m.	Thermometer. London: Roy. Soc. Fahr. 9 a.m.	Thermometer. London: Roy. Soc. Self-register. Max.	Thermometer. London: Roy. Soc. Self-register. Min.	Thermometer. Chiswick. Max.	Thermometer. Chiswick. Min.	Thermometer. Boston. 8¾ a.m.	Thermometer. Dumfries-shire. 9 a.m.	Thermometer. Dumfries-shire. 9 p.m.	Wind. London: Roy. Soc. 9 a.m.	Wind. Chiswick 1 p.m.	Wind. Bost.	Wind. Dumfries-shire.	Rain. London: Roy. Soc. 9 a.m.	Rain. Chiswick.	Rain. Boston.	Rain. Dumfries-shire.	Dew-point. Lond.: Roy. Soc. 9 a.m.
1.	30·030	30·045	29·892	29·54	29·75	29·60	46·3	47·7	39·7	50	39	44·0	44	44	E.	SW.	calm	SW.	·041	...	...	...	39
2.	29·868	30·012	29·886	29·40	29·88	30·08	45·7	46·3	42·8	57	38	44	41	36	SSE.	SW.	calm	SW.	...	...	...	...	39
3.	30·068	30·088	30·071	29·63	30·20	30·18	43·7	53·3	41·6	52	35	42	38	35	N.	NE.	calm	E.	...	...	...	...	40
4.	30·052	30·118	30·057	29·73	30·20	30·27	40·3	50·3	36·8	52	33	37·5	38	35½	NE.	NE.	E.	E.	...	...	...	...	36
5.	30·108	30·126	30·058	29·80	30·30	30·29	35·2	37·0	33·8	35	30	34	35½	33	NE.	NE.	calm	E.	...	...	...	...	32
6.	29·854	29·894	29·726	29·62	30·13	30·00	32·5	32·8	31·6	35	25	35	30	26	NE.	NE.	E.	E.	...	·06	...	1·18	29
7.	29·526	29·624	29·543	29·33	29·93	29·98	30·7	31·7	27·4	35	29	31	29½	25	NW.	NW.	E.	NE.	...	...	...	...	26
☾ 8.	29·748	29·981	29·769	29·48	30·04	30·16	32·2	34·7	30·0	38	21	31	30	25½	NW.	N.	E.	E.	...	...	...	...	28
9.	29·948	30·139	29·987	29·65	30·18	30·15	29·8	30·2	26·0	40	20	32	27	27	NNW.	NW.	calm	N.	...	...	...	...	25
10.	30·172	30·238	30·196	29·85	30·15	30·18	30·5	35·8	26·5	42	30	27·5	30	30	E.	SE.	NW.	SW.	...	...	...	...	23
11.	30·182	30·216	30·163	29·92	30·09	30·13	36·5	37·8	29·9	46	32	35	33	37½	E.	SE.	SE.	SE.	...	...	...	...	28
12.	30·074	30·114	30·064	29·84	30·15	30·10	35·4	36·3	32·8	47	37	35	36½	34	NE var.	SE.	E.	E.	...	...	...	...	29
13.	30·048	30·074	30·041	29·72	30·05	30·00	40·7	43·5	34·8	55	41	38	39½	41½	NE.	SE.	E.	SE.	·022	·30	·66	...	34
14.	30·014	30·076	30·023	29·64	30·00	29·90	45·7	50·2	40·7	55	44	40	40½	43	SE.	S.	E.	SE.	·216	·39	·47	0·28	36
● 15.	29·806	29·848	29·396	29·35	29·70	29·38	46·7	47·5	45·6	52	40	46	43½	42½	S.	SW.	S.	SSE.	·327	·29	·23	...	42
16.	29·260	29·314	29·261	28·80	29·29	29·42	46·8	50·8	42·3	54	36	45	42½	37	SW.	SW.	W.	NNW.	·250	·10	·56	...	43
17.	29·464	29·821	29·482	29·10	29·89	30·10	39·8	51·2	38·0	43	34	39	37	42	NE.	E.	calm	N by E.	·044	...	·03	...	36
18.	29·880	30·021	29·805	29·53	30·12	30·05	35·8	41·0	34·8	37	27	35	36	36½	NW.	NE.	NE.	S by E.	...	...	...	...	33
19.	30·022	30·064	30·026	29·65	30·08	30·04	37·0	38·0	33·3	49	29	36	38½	40	W.	NW.	calm	WSW.	...	...	...	...	32
20.	29·968	30·003	29·712	29·56	29·80	29·53	41·2	41·7	36·2	49	41	37·5	40½	43	W.	SW.	calm	SSW.	...	·09	...	...	35
21.	29·616	29·651	29·601	29·15	29·64	29·51	46·7	48·2	41·2	56	40	46	42	39	W.	W.	calm	S.	·077	·07	...	...	40
☽ 22.	29·700	29·733	29·690	29·12	29·50	29·59	47·2	48·8	42·4	47	43	42	37½	40	SW.	W.	NW.	NW.	·016	...	·11	...	44
23.	29·700	29·717	29·661	29·18	29·43	29·50	50·5	51·2	44·3	55	45	47·5	46	41	S.	SW.	W.	W.	...	·07	...	...	43
24.	29·682	29·680	29·656	29·13	29·52	29·46	47·4	48·3	46·8	58	40	47·5	41	36	S.	W.	calm	SW.	·088	...	...	2·60	44
25.	29·648	29·706	29·634	29·08	29·48	29·65	47·7	49·3	41·8	53	37	44	39	36	S.	W.	W.	W.	...	·03	...	...	43
26.	29·810	29·946	29·771	29·40	29·95	29·78	44·2	45·2	41·7	52	39	40	35½	40½	NE.	E.	W.	S.	...	·05	...	...	39
27.	29·670	29·667	29·343	29·15	29·55	29·37	48·0	48·8	42·2	54	41	45·5	48	43½	S.	W.	S.	SW.	·044	·14	·14	...	42
28.	29·386	29·523	29·402	28·80	29·35	29·56	48·8	51·5	42·7	54	39	48	44	37	S.	NE.	calm	N by E.	·133	·20	·09	...	44
29.	29·454	29·448	29·419	29·10	29·66	29·77	43·7	53·2	40·3	47	31	42	37½	33	NNW.	NE.	calm	E.	·291	...	·30	...	42
○ 30.	29·762	29·800	29·770	29·45	29·68	29·75	41·2	45·7	35·0	45	37	40	35½	37	ENE.	NE.	E.	E.	...	...	...	...	35
31.	29·612	29·614	29·543	29·32	29·66	29·67	41·0	43·0	37·2	48	40	42	39½	37	E.	SE.	E.	ESE.	...	·16	...	...	37
Mean.	29·811	29·880	29·763	29·42	29·662	29·195	41·3	44·2	37·4	44·84	35·58	39·6	37·8	36·6					Sum. 1·549	1·95	2·59	4·06	Mean. 36·1

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LX. *Experiments upon the Products of Respiration at different Periods of the Day.* By CHARLES T. COATHUPE, Esq.*

THE apparatus employed in the following experiments for ascertaining the amount of carbonic acid gas respired, differs in its construction from any that has hitherto been used for a similar purpose; but (although original in its construction,) it may claim advantages, both as regards simplicity and utility, which will most probably be admitted by those who are conversant with gaseous manipulations.

It consists of a cylindrical glass tube $24\frac{1}{2}$ inches long, and 0·55 inch in diameter, terminated at each extremity by a brass cap, into which a stop-cock may be screwed. The glass tube is accurately divided into 175 equal parts, by equal measures of mercury, and the divisions are numbered on opposite sides of the tube in such a manner, that let either end be uppermost, the graduations may be instantly read. Every experiment may thus be tested by a double reading, by simply inverting the tube, and waiting a few seconds until the liquid employed for any examination has drained to its ultimate level.

The total capacity of the tube is about 5·33 cubic inches, and each division is separated by a space exceeding $\frac{1}{8}$th of an inch.

The reagent used was a clear saturated solution of quicklime in distilled water, with which a preliminary experiment was made for the purpose of ascertaining the extent of atmo-

* Communicated by the Author.

spheric tension, to supply the necessary corrections in an easy and sufficiently practical manner.

The experiment and results were as follows:

The stop-cocks being firmly screwed into the brass caps, each in its own place as marked previously to the graduation of the tube, and both plugs turned for the free admission of air, the one extremity was immersed in lime-water and the lips were applied to the other; then gently sucking the air from the tube until the lime-water had reached within 4 or 5 inches of the upper extremity, the upper stop-cock was closed, and subsequently the lower one.

The lower surface of the lime-water appeared between the divisions 36 and 37. Now opening the upper cock to relieve the tension, closing it again and opening the lower cock, a small quantity of the lime-water escaped, after which the lower cock was closed, and the lower surface of the lime-water appeared between the divisions 38 and 39.

By repeating these alternate restorations of the tension exercised upon the air included within the tube, the following results were obtained, taking in all cases the nearest approximation to either 4ths or 3rds of a whole division.

Table of Tension.

Barometer 30·2 inches. Thermometer 48° Fahrenheit.

1st, air under tension.				Air under subsequent tension.		
*37·5 parts	became after relieving tension			39·5 parts		difference 2.
39·5	do.	do.	do.	41·5	do.	
41·5	do.	do.	do.	43·75	do.	
43·75	do.	do.	do.	46·	do.	2·25
46·	do.	do.	do.	48·25	do.	
48·25	do.	do.	do.	50·5	do.	
50·5	do.	do.	do.	53·	do.	
53·	do.	do.	do.	55·5	do.	
55·5	do.	do.	do.	58·	do.	2·5
58·	do.	do.	do.	60·5	do.	
60·5	do.	do.	do.	63·	do.	

From 63 to 125 the differences were three divisions or parts, upon each remission, and subsequent restoration of tension;
— from 125 to $130\frac{1}{2}$, the differences were 2·75 parts each;
— from 130·5 to 138, they were 2·5 parts, ... do.
— from 138 to 145, they were 2·33 parts... ... do.
— from 145 to 151, they were 2 parts ... ... do.

* From the graduation stated as 37·5 parts, to the terminating orifice of the lower stop-cock, the column of lime-water measured 21·25 inches. From plug of upper stop-cock to 37·5° = 6·25 inches.

The air respired was in most cases retained in a caoutchouc ("Mackintosh") bag furnished with a brass stop-cock and connecter, and capable of containing 1000 cubic inches. It was invariably filled by successive respirations, and great care was taken to obviate the possibility of inspiring any air that had been once respired.

The mode of conducting the experiments was as follows:—

The caoutchouc bag having been filled with respired air, the long graduated tube (with its stop-cocks annexed) having been swilled out with distilled water, and drained, and a small evaporating vessel filled with lime-water, the tube with its stop-cocks was screwed to the connecter of the stop-cock of the caoutchouc bag. The bag was laid upon a chair, while with the knee considerable pressure was exerted upon it, and having opened all the communications between the interior of the bag and the atmospheric air, the pressure upon the bag was continued until about 200 cubic inches of its contents had passed through the tube. The cock at the further extremity of the tube was now closed, and the pressure upon the bag being continued with increased force, the cock at the nearer extremity of the tube was closed. Having secured the stop-cock of the caoutchouc bag, the tube with its own stop-cocks was disconnected. (It is evident that the air included within the tube must be under pressure; and the object for producing this effect is, that the small quantity of atmospheric air that is retained below the plug of the cock shall be displaced when the extremity of the cock is immersed in the lime-water. The accomplishment of this object is fully known by the gurgling noise that accompanies the opening of the cock after its immersion.)

The stop-cock terminating one of the extremities of the tube was now inserted into the vessel that contained the lime-water: the plug was turned for the free egress of the atmospheric air retained below it, and for the subsequent ingress of the reagent; the lips were applied to the orifice of the upper stop-cock, and the process of exhaustion having been commenced, the upper plug was gently turned, and the exhaustion steadily continued until the reagent had risen so as to occupy about 30 or 40, or more, of the lower divisions of the tube. The upper stop-cock was then secured, and after a moment's pause the lower one was secured. The number of divisions unoccupied by the lime-water were now noted, the tube being held upright, and the lower surface of the water observed.

The finger was applied to the lower orifice of the cock which had been immersed, in order to retain that portion of

the lime-water which occupied the space below the plug. The tube with its contents were well agitated, the finger was removed from the orifice of the stop-cock, the same extremity was again immersed in the lime-water, and the plug of the cock gently turned.

As soon as the lime-water had risen within the tube to its fixed level, the cock was closed again; the finger replaced upon the lower orifice, and agitation renewed.

This process was repeated until the lime-water ceased to absorb any of the air contained within the tube. Two operations, however, were generally sufficient. The tube was then hanged up by a loop of wire against a corner of the wall, and when the interior had drained to a permanent level, the upper cock was opened; a small quantity of air immediately rushed into the tube to relieve the tension, and with it generally a single drop of lime-water that had occupied the small space within the bore of the upper cock beneath its plug.

The difference between the number of divisions, or parts, first observed to be unoccupied by the lime-water, and the number of parts lastly observed to be unoccupied, is the number of parts of carbonic acid gas that was contained in the number of parts first observed.

As these experiments were commenced and concluded with as little delay as possible, it was not thought necessary to register the height of the barometer, nor the temperature; but every precaution was taken to commence and conclude every set of experiments from the same bag of air, under precisely similar circumstances. The tube, the bag of air, and the lime-water were invariably of the same temperature, both before and after each experiment.

The periods selected for the experiments were,

From 8 a.m. to 9½ a.m. before breakfast.
9½ a.m. to 12 noon } during the digestion of break-
12 noon to 1 p.m. } fast and before "luncheon."
1 p.m. to 5½ p.m. before dinner.
5½ p.m. to 8¾ p.m. during the digestion of dinner.
8¾ p.m. to 12 night.

Habits of the operator.

At 9½ a.m. a slender breakfast.
1 p.m. to 2 p.m. do. "luncheon."
5½ p.m. a good dinner, with a pint of wine.
8½ p.m. one small cup of tea.
10 p.m. occasionally one glass of weak brandy and water.
12 to 1. bed time.

Age, 38 years. Stature, 5 feet 8 inches. Weight about 140 pounds.

Average pulse, 60 to 62 per minute. Average inspiration, 18 to 21 per minute.—Vide p. 409, Epitome.

First Interval, from 8 a.m. to $9\frac{1}{2}$ a.m. (before breakfast.)

Feb. 4th.	at $8\frac{1}{2}$ a.m.	137 parts of respired air, became		130·	∴ loss 5·11	per cent.
„	at 9 a.m.	124·5	do.	118·25	∴ do. 5·02	do.
„	„ $9\frac{1}{2}$ a.m.	111·25	do.	105·25	∴ do. 5·39	do.
5th.	„ 8 a.m.	108·	do.	99·5	∴ do. 7·87	do.
„	„ $8\frac{1}{2}$ a.m.	128·75	do.	120·	∴ do. 6·79	do.
„	„ $9\frac{1}{2}$ a.m.	119·	do.	114·5	∴ do. 3·80	do.
6th.	„ 8 a.m.	76·5	do.	72·5	∴ do. 5·22	do.
„	„ 9 a.m.	131·25	do.	124·75	∴ do. 4·95	do.
„	„ $9\frac{1}{2}$ a.m.	109·5	do.	105·	∴ do. 4·11	do.
„	„ „	122·25	do.	118·	∴ do. 3·47	do.
„	„ „	105·	do.	100·	∴ do. 4·76	do.
7th.	„ 8 a.m.	112·5	do.	108·5	∴ do. 4·00	do.
„	„ $9\frac{1}{4}$ a.m.	105·	do.	100·5	∴ do. 4·28	do.
8th.	„ 8 a.m.	81·	do.	78·25	∴ do. 3·40	do.
„	„	92·5	do.	89·	∴ do. 3·78	do.
„	„	92·5	do.	89·	∴ do. 3·78	do.
„	„	87·	do.	84·	∴ do. 3·45	do.
„	„ 9 a.m.	113·	do.	108·25	∴ do. 4·20	do.
„	„	112·25	do.	108·	∴ do. 3·78	do.
9th.	at 8 a.m.	124·	do.	120·	∴ do. 3·22	do.
„	„	121·	do.	117·	∴ do. 3·30	do.
„	„	132·5	do.	128·5	∴ do. 3·02	do.
„	„ $9\frac{1}{4}$ a.m.	115·3	do.	112·6	∴ do. 2·31	do.
12th.	„ 8 a.m.	116·75	do.	112·50	∴ do. 3·64	do.
„	„	120·5	do.	115·80	∴ do. 3·90	do.
„	„	121·5	do.	116·80	∴ do. 3·86	do.
„	„ $9\frac{1}{2}$ a.m.	133·	do.	127·50	∴ do. 4·13	do.
„	„	108·	do.	103·50	∴ do. 4·17	do.
„	„	104·	do.	99·80	∴ do. 4·04	do.
13th.	„ 8 a.m.	99·	do.	94·00	∴ do. 5·00	do.
„	„	104·	do.	99·50	∴ do. 4·32	do.
„	„	122·	do.	116·50	∴ do. 4·90	do.
Total of 32 exp.		3590·3	. . . . .		3437·25	
		3437·25				

∴ 153·05 parts were absorbed by lime-water.

and 3590·3 : 153·05 : : 100 : 4·262 per cent.

The average therefore of 32 experiments made before breakfast, and comprising 8 days, appears to indicate 4·262 per cent. of carbonic acid gas in the air respired.

If we correct the above for tension, as per table at page 402, it will become about 4·37 per cent.

N.B. The average correction for tension, computed from

an average of 124 experiments, gives 0·11 per cent. additive to the apparent per centage of carbonic acid gas as derived from any direct experiment.

Second Interval. $9\frac{1}{2}$ a.m. to 12 noon.

Feb. 5th. at 11 a.m.	134 parts of respired air, became		131·	∴ loss 2·24	per cent.
4th. „ 11 a.m.	110·	do.	105·	∴ do. 4·54	do.
„ „ 12 noon	118·5	do.	113·25	∴ do. 4·33	do.
6th. „ $10\frac{1}{4}$ a.m.	110·25	do.	105·75	∴ do. 4·08	do.
„ „ $11\frac{3}{4}$ a.m.	131·5	do.	124·75	∴ do. 5·13	do.
7th. „ $11\frac{1}{4}$ a.m.	100·5	do.	97·5	∴ do. 2·98	do.
8th. „ $11\frac{1}{2}$ a.m.	128·5	do.	124·	∴ do. 3·5	do.
„ „	109·	do.	104·5	∴ do. 4·12	do.
„ „	118·	do.	113·	∴ do. 4·23	do.
„ „	115·	do.	110·25	∴ do. 4·13	do.
9th. „ $11\frac{1}{2}$ a.m.	106·	do.	102·5	∴ do. 3·30	do.
„ „	93·3	do.	90·3	∴ do. 3·22	do.
12th. „ $11\frac{3}{4}$ a.m.	116·75	do.	112·3	∴ do. 3·81	do.
„ „	110·5	do.	106·3	∴ do. 3·80	do.
„ „	96·0	do.	93·0	∴ do. 3·12	do.
Total of 15 exp.	1697·8	. . . .		1633·4	
	1633·4				

∴ 64·4 parts were absorbed by the lime-water.
and 1697·8 : 64·4 : : 100 : 3·79 per cent.

The average therefore of 15 experiments upon the air respired between 10 a.m. and noon, and comprising 7 days, indicates 3·79 per cent. of carbonic acid gas, which corrected for tension will be 3·90 per cent.

Third Interval. 12 noon to 1 p.m.

Feb. 5th. at $12\frac{1}{4}$ p.m.	100· parts of respired air, became		96·5	∴ loss 4·5	per cent.
6th. „ $12\frac{3}{4}$ p.m.	128·	do.	122·	∴ do. 4·68	do.
7th. „ 1 p.m.	121·	do.	117·5	∴ do. 3·90	do.
8th. „ $12\frac{1}{2}$ p.m.	121·5	do.	116·75	∴ do. 3·90	do.
12th. „ 1 p.m.	106·	do.	101·66	∴ do. 4·10	do.
„ „	115·5	do.	111·5	∴ do. 3·46	do.
„ „	110·5	do.	106·	∴ do. 4·07	do.
Total of 7 exp.	802·5	. . . .		771·91	
	771·91				

∴ 30·59 parts were absorbed by the lime-water.
and 802·5 : 30·59 : : 100 : 3·81 per cent.

The average therefore of 7 experiments upon the air respired between 12 noon and 1 p.m., and comprising 5 days,

indicates 3·81 per cent. of carbonic acid gas, which corrected for tension will be 3·92 per cent.

Fourth Interval, from 1 p.m. to $5\frac{1}{2}$ p.m. (before dinner.)

Jan. 31.	at	2 p.m.	144·5	parts of air respired, became	135·	∴ loss 6·57	per cent.
"		"	148·	do.	138·	∴ do. 6·75	do.
"	"	3 p.m.	135·	do.	128·5	∴ do. 4·81	do.
Feb. 4th.	"	$3\frac{3}{4}$ p.m.	99·5	do.	97·	∴ do. 2·5	do.
"	"	$5\frac{1}{2}$ p.m.	100·5	do.	94·5	∴ do. 6·0	do.
5th.	"	5 p.m.	98·	do.	93·	∴ do. 5·1	do.
6th.	"	5 p.m.	97·5	do.	92·5	∴ do. 5·12	do.
7th.	"	$2\frac{1}{4}$ p.m.	114·5	do.	109·5	∴ do. 4·36	do.
"	"	$2\frac{1}{2}$ p.m.	99·	do.	96·75		
"		"	99·	do.	96·75	∴ do. 2·25	do.
"		"	100·	do.	97·75		
"		"	100·	do.	98·		
"	"	$3\frac{1}{2}$ p.m.	99·5	do.	95·	∴ do. 4·5	do.
"	"	$3\frac{3}{4}$ p.m.	116·	do.	112·75		
"		"	98·	do.	93·5		
"		"	115·	do.	111·25	∴ do. 3·56	do.
"		"	96·5	do.	93·		
"		"	112·75	do.	109·		
"		"	86·	do.	82·75		
"	"	5 p.m.	105·	do.	101·5	∴ do. 3·33	do.
"	"	$5\frac{1}{4}$ p.m.	131·	do.	128·5	∴ do. 1·90	do.
8th.		4 p.m.	111·	do.	105·75	∴ do. 4·73	do.
"	"	5 p.m.	94·	do.	91·5	∴ do. 2·66	do.
11th.		$5\frac{1}{2}$ p.m.	106·75	do.	102·	∴ do. 4·45	do.
"		"	99·5	do.	95·3	∴ do. 4·22	do.
"		"	119·4	do.	113·5	∴ do. 4·94	do.
12th.		5 p.m.	117·66	do.	112·5	∴ do. 4·38	do.
"		"	104·3	do.	99·5	∴ do. 4·60	do.
"		"	120·5	do.	115·	∴ do. 4·56	do.
Total of 29 exp.			3168·36	. . . . .		3039·55	
			3039·55				

∴ 128·81 parts were absorbed by the lime-water.
and 3168·36 : 128·81 : : 100 : 4·06 per cent.

The average therefore of 29 experiments upon the air respired between 2 p.m. and $5\frac{1}{2}$ p.m., and comprising 8 days, indicates 4·06 per cent. of carbonic acid gas, which corrected for tension will be 4·17 per cent.

Fifth Interval, from $5\frac{1}{2}$ p.m. to $8\frac{3}{4}$ p.m. (during digestion.)

Jan. 31st. at $8\frac{1}{2}$ p.m.	124·	parts of respired air, became	121·	∴ loss 2·42	per cent.
Carried forward	124·			2·42	

Brought forward	124·			2·42	
Feb. 3rd. at $7\frac{3}{4}$ p.m.	132·5	air respired	126·5	∴ loss 4·54	per cent.
4th. „ $7\frac{1}{4}$ p.m.	110·	do.	106·	∴ do. 3·64	do.
„ „ $8\frac{1}{4}$ p.m.	104·25	do.	100·5	∴ do. 3·59	do.
5th. „ 7 p.m.	114·5	do.	110·	∴ do. 3·93	do.
„ „ 8 p.m.	103·	do.	99·	∴ do. 3·88	do.
6th. „ $7\frac{1}{2}$ p.m.	83·5	do.	82·	∴ do. 1·80	do.
„ „ $8\frac{1}{2}$ p.m.	119·5	do.	116·25	∴ do. 2·72	do.
7th. „ $8\frac{1}{2}$ p.m.	101·	do.	96·	∴ do. 4·95	do.
„ „	97·25	do.	94·	∴ do. 3·34	do.
11th. „ $8\frac{1}{2}$ p.m.	125·2	do.	120·5	∴ do. 3·75	do.
„ „	116·	do.	112·	∴ do. 3·45	do.
„ „	121·	do.	116·3	∴ do. 3·88	do.
12th. „ $8\frac{1}{4}$ p.m.	120·	do.	116·	∴ do. 3·33	do.
„ „	107·5	do.	104·	∴ do. 2·80	do.
„ „	110·5	do.	107·	∴ do. 3·16	do.
„ „ $8\frac{1}{2}$ p.m.	118·5	do.	114·	∴ do. 3·81	do.
Total of 17 exp.	1908·2	. . . .		1841·05	
	1841·05				

∴ 67·15 parts were absorbed by the lime-water.
and 1908·2 : 67·15 : : 100 : 3·52 per cent.

The average therefore of 17 experiments upon the air respired between 7 p.m. and $8\frac{1}{2}$ p.m., and comprising 8 days, indicates 3·52 per cent. of carbonic acid gas, which, corrected for tension, will be 3·63 per cent.

Sixth Interval, from $8\frac{3}{4}$ p.m. to 12 night.

Feb. 3rd. at 12 night	135·	parts of respired air, became	126·5	∴ loss 6·3	per cent.
4th. „ $11\frac{1}{4}$ p.m.	117·5	do.	111·5	∴ do. 5·1	do.
„ „ 12 night	124·	do.	118·5	∴ do. 4·43	do.
„ „	97·	do.	92·5	∴ do. 4·64	do.
5th. „ $11\frac{1}{2}$ p.m.	127·5	do.	121·5	∴ do. 4·7	do.
„ „ 12 night	113·	do.	109·	∴ do. 3·54	do.
6th. „ $9\frac{3}{4}$ p.m.	126·	do.	121·5	∴ do. 3·57	do.
„ „ $11\frac{1}{4}$ p.m.	122·	do.	118·5	∴ do. 2·87	do.
7th. „ 9 p.m.	96·75	do.	94·25	∴ do. 2·60	do.
„ „ $11\frac{1}{4}$ p.m.	133·	do.	127·	∴ do. 4·51	do.
„ „ $11\frac{1}{2}$ p.m.	111·	do.	106·5	∴ do. 4·05	do.
8th. „ $10\frac{1}{2}$ p.m.	105·	do.	102·	∴ do. 2·85	do.
„ „	100·	do.	96·	∴ do. 4·00	do.
„ „	110·5	do.	107·5	∴ do. 2·71	do.
„ „	97·5	do.	94·	∴ do. 3·59	do.
„ „ 12 night	111·5	do.	106·75	∴ do. 4·26	do.
„ „	95·5	do.	91·25	∴ do. 4·43	do.
„ „	96·	do.	92·	∴ do. 4·17	do.
Carried forward	2018·77	.	1936·75		

Brought forward	2018·77	.	1936·57		
Feb. 11th. at $11\frac{1}{2}$ p.m.	124·3	air respired	119·5	∴ do. 3·86	do.
„ „	112·66	do.	107·3	∴ do. 4·75	do.
„ „	107·66	do.	102·	∴ do. 5·25	do.
12th. „ 11 p.m.	133·	do.	129·	∴ do. 3·00	do.
„ „	116·5	do.	113·	∴ do. 3·00	do.
„ „	115·5	do.	111·5	∴ do. 3·47	do.
Total of 24 exp.	2728·37	-	2619·05		
	2619·05				

∴ 109·32 parts were absorbed by the lime-water.

2728·37 : 109·32 : : 100 : 4·01 per cent.

The average therefore of 24 experiments upon the air respired between 9 p.m. and midnight, and comprising eight days, indicates 4·01 per cent. of carbonic acid gas, which corrected for tension will be 4·12 per cent.

The average of all the experiments together, 124 in number, and comprising almost every hour of the day between 8 a.m. and midnight, and including a period of 8 days, gives

13895·53 : 553·32 : : 100 : 3·9819 per cent., which when corrected for tension, indicates 4·09 per cent. as the total daily average of the carbonic acid gas in the air respired from the lungs.

Epitome of the Results.

1st period	8 a.m. to $9\frac{1}{2}$ a.m.	32	exp.	indicated	4·37	per cent. of carb. acid gas.
2nd do.	10 a.m. to 12 noon	15	do.	do.	3·90	
3rd do.	12 noon to 1 p.m.	7	do.	do.	3·92	
4th do.	2 p.m. to $5\frac{1}{2}$ p.m.	29	do.	do.	4·17	
5th do.	7 p.m. to $8\frac{1}{2}$ p.m.	17	do.	do.	3·63	
6th do.	9 p.m. to midnight	24	do.	do.	4·12	
		124 exp. comprising 8 days.				

Hence we find the carbonic acid gas produced by respiration to be a variable quantity, that it is less during the period of active digestion, that it increases with increased abstinence from food; and that it varies in the same individual at *similar* periods of *different* days. It also appeared during these experiments, that *excitement of any kind* (whether from the exhilarating stimulus of wine, or from the irritating annoyances which are wont to occur to most folks who are actively engaged,) caused a diminution of carbonic acid in the air respired, as compared with the ordinary average of that respired at a similar period of the day, and during a state of ordinary tranquillity. *The total daily average* indicated 4·09

per cent. of carbonic acid gas. The *maximum* observed at any single examination was 7·98 per cent. It was at 8 a.m. February 5th. The *minimum* observed at any single examination was 1·91 per cent. It was at $7\frac{1}{2}$ p.m. February 7th.

C. T. C., February 14th, 1839.

Having referred to some very interesting experiments made by Messrs. Allen and Pepys at the laboratory of the Royal Institution, and recorded in the "Philosophical Transactions of the Royal Society," published in the year 1809, I was much astonished to find results differing widely from my own; and therefore, with a view to ascertain the cause of these differences, I tested my own results by a few collateral experiments, which were as follows:

Feb. 18. Air respired into a bag at 8 a.m.

No. 1.	121·25 parts	became 117. ∴ loss	= 3·51	per cent.
No. 2.	89·25 do.	do. 86. ∴ do.	3·64	
			2)7·15	
		∴ average loss	= 3·57 p. cent.	

which, corrected for tension, will be 3·68 per cent.

32 parts of the residual air from No. 1. experiment, after the abstraction of the carbonic acid gas by lime-water, were mixed with 13 parts of dry hydrogen gas received over mercury. The eudiometer contained 45 parts, the interior and exterior surfaces of the mercury being adjusted to the same level.

The thermometer stood at 47° Fahrenheit. The barometer at 29·5 in. a Dobereiner's ball of spongy platina was introduced into the mixture, and it absorbed 16·75 parts.

Therefore the 32 parts of residual air contained 5·58 parts of oxygen; and 100 parts of the respired air, minus the 3·68 parts of carbonic acid gas abstracted by the lime-water = 96·32 parts, must contain

Oxygen.
32 : 5·58 : : 96·32 : 1679 parts oxygen.

and 16·79 parts of oxygen + 3·68 parts of carbonic acid gas = 20·47 parts represent the total quantity of oxygen that appeared to have existed in the atmospheric air previous to its having been respired.

Now as it is very well known that ordinary atmospheric air contains 21 per cent. of oxygen and 79 per cent. of nitrogen, and that the formation of any quantity of carbonic acid gas must necessarily require precisely its own volume of oxygen; also, that air when emanating from the moist surface of the cells of the lungs must contain 1·21 per cent. of aqueous vapour at

47° Fahrenheit when the barometer stands at 30 inches; we demonstrate the constituents of 100 parts of air which had passed through the lungs for this test experiment to be

16·79	parts oxygen.
3·68	do. carbonic acid gas.
1·23	do. aqueous vapour at 47° Fahr.; barom. 29·5 in.
78·35	do. nitrogen, (being the ordinary quantity in 100 parts of air that contain 1·23 parts of aq. vap.)
100·05	

Consequently there was no error in the abstraction of the carbonic acid.

Feb. 19. Air respired into a bag, at $9\frac{1}{2}$ a.m.

110·5 parts,	became	105·5	∴	4·52	per cent. loss.	
124·33 do.	do.	119·33	∴	4·02	do.	
				2)8·54		
		∴ average loss =		4·27	per cent.	
which, corrected for tension, will be				4·38	per cent.	

100 parts taken from the same bag over mercury. Barometer at 29·5 inches, and temperature 47° Fahrenheit.

At 11 a.m. a piece of caustic potassa was introduced, and after a lapse of 61 hours it was abstracted.

5 parts had been absorbed, the thermometer being 47° Fahrenheit, and the barometer standing at 29·65 inches. The residue

95 parts; barometer at 29·65 inches, are equal to
95·48 do. do. at 29·5 inches,

consequently, only 4·52 parts of the original quantity had been absorbed, which will include both the carbonic acid gas and the aqueous vapour. Now as the aqueous vapour at this temperature and pressure would be 1·23 per cent. there will remain only 3·29 for the carbonic acid gas; a result which shows some error in the experiment, but still tends to prove that the abstraction of the carbonic acid gas by the means adopted throughout these experiments, viz. by a long and comparatively wide tube with lime-water as the reagent, are not less efficient than a small and closely graduated tube with caustic potassa for the reagent, even although the latter be employed over mercury.

Having no ostensible reason for mistrusting my previous experiments, I began to reflect upon the probable sources of error in those of Messrs. Allen and Pepys; and here I will detail the most important circumstances attending their researches upon the subject in question.

1st. Their experiments appear to have been made with

great care, with every reasonable precaution, and with the honest intention of reporting the truth undisguised.

2nd. Their apparatus was good of its kind. It belonged to the laboratory of the Royal Institution.

3rd. Their reagent was lime-water.

4th. Their eudiometer consisted of a tube containing *one cubic inch*, graduated into 100 *equal parts.*

5th. The time required for obtaining their average supply of respired air was generally from 10 to 11 minutes.

6th. The operator's ordinary respirations were 19 *per minute*, but during the period necessary for obtaining the quantity of respired air upon which Messrs. Allen and Pepys bestowed their attention, they amounted to only 5 *per minute.*

7th. All their experiments were made either *before breakfast*, or *immediately before dinner.*

8th. Their average of "*carbonic acid gas produced by the process of respiration*," amounted to 8 *per cent.* upon the air respired *.

To prove whether there could be any difference in the quantity of carbonic acid gas produced by protracting the usual time of natural respiration, two bags were filled, the one immediately after the other, with air respired as follows:

$$\left.\begin{array}{l}\text{No. 1. Bag, by respiring 18 times per minute}\\ \text{No. 2. Bag, by respiring 4 times per minute}\end{array}\right\}\text{at 9 a.m.}$$

No. 1. Bag.	124·5 parts,	became	120·33	∴ loss	=	3·34	p. cent.
	116·33 do.	do.	112·33	∴ do.	=	3·43	do.
	119·8 do.	do.	115·66	∴ do.	=	3·45	do.
						3)10·22	
			∴ average loss......			3·41	p. cent.

which, corrected for tension, will be 3·52 per cent.

No. 2. Bag.	110·5 parts,	became	105·5	∴ loss	=	4·52	
	124·33 do.	do.	119·33	∴ do.		4·02	do.
						2)8·54	
			∴ average loss		=	4·27	p. cent.

which, corrected for tension, will be 4·38 per cent.

Feb. 19th.

Air respired at 9 p.m. Feb. 19th.

$$\left.\begin{array}{l}\text{No. 1. Bag, filled, respiring 18 times per minute.}\\ \text{No. 2. do. do. do. 3 times per minute.}\end{array}\right\}$$

* Messrs. Allen and Pepys's Researches in Respiration will be found in Phil. Mag. First Series, vol. xxxii. p. 242.—Edit.

No. 1.	99·5 parts,	became	95·5	∴ loss =	4·02	per cent.
	96·5 do.	do.	93·	∴ do.	3·62	do.
					2)7·64	
				∴ average loss =	3·82	per cent.

which, corrected for tension, will be 3·93 per cent.

No. 2. Bag.	101 parts,	became	95·75	∴ loss =	5·20	p. cent.
	113·5 do.	do.	108·	∴ do.	4·84	do.
	126 do.	do.	120	∴ do.	5·00	do.
					3)15·04	
				∴ average loss =	5·01	p. cent.

which, corrected for tension, will be 5·12 per cent.

These experiments, made during the same day, and at an interval of exactly 12 hours, the one confirming the other, show that an increase of carbonic acid gas is eliminated by protracting the respiratory process, which increase (from the average of the present experiments) amounted to one fourth more than the quantity of carbonic acid gas found in natural respirations.

These results show at once the expediency of reducing the quantity of carbonic acid gas obtained by the experiments of Messrs. Allen and Pepys by one fifth part. The 8 per cent. will then become 6·4 per cent.

Considering then that Messrs. Allen and Pepys's operations were carried on at periods of the day in which I have shown the respiration to contain its *maximum* quantity of carbonic acid gas, viz. *before breakfast*, and *immediately before dinner*, and that, although the absolute volume of respired air upon which they operated was very considerable, yet it was the volume obtained at *one* period of the day; and that although their experiments upon this large volume of air were very numerous, yet the volumes themselves were not renewed above thrice; it appears that we may rely upon their demonstration that the air which they examined would, if respired in the natural manner, have contained 6·4 per cent. of carbonic acid gas, which quantity is almost the maximum that occasionally occurs at the most favourable periods for its production.

Having very many times examined the air respired by other individuals, I have never yet met with a single instance which did not accord with the averages already detailed.

From my own observations, and from the experiments of others, the following details connected with this subject may be faithfully relied on, viz.

1st. The average number of respirations made by most adult healthy individuals (varying from 17 to 23 per minute) may be stated as 20 per minute.

2nd. The average bulk of air respired at each respiration made by such individuals (varying from 14 to 18 cubic inches) may be stated as 16 cubic inches.

3rd. The average daily amount of carbonic acid gas found in the air respired by such individuals (varying at its extremes from 1·9 to 7·98 per cent.) may be stated as 4 per cent.

Hence 460800 cubic inches, or 266·66 cubic feet of air pass through the lungs of a healthy adult of ordinary stature in 24 hours, of which 10·666 cubic feet will be converted into carbonic acid gas, = 2386·27 grs. or 5·45 ounces avoirdupois, of carbon. This gives 99·6 grs. of carbon per hour, produced by the respiration of one human adult, or 124·328 pounds annually; and if we multiply this by $26\frac{1}{2}$ millions (being the calculated population of Great Britain and Ireland for the year 1839) we have 147,070 tons of carbon as the annual product of the respiration of human beings at present existing within the circumscribed boundaries of Great Britain and Ireland.

Hence also the *maximum quantity* of fresh atmospheric air that can possibly be required by a healthy adult during 24 hours, even supposing that no portion of the air respired could be again inspired, will not exceed 266·666 *cubic feet*.

Wraxall, near Bristol, Feb. 1839. CHARLES THORNTON COATHUPE.

LXI. *On the Decomposition of Amygdalin by Emulsin.* By ROBERT D. THOMSON, *M.D., and Mr.* THOMAS RICHARDSON.*

SOME years ago Robiquet and Boutron-Charlard showed that volatile oil of bitter almonds and prussic acid, which are obtained by the distillation of bitter almonds, do not exist naturally in the almonds but result from the process. They further ascertained, that when milk of bitter almonds, formed by triturating almonds with water in a mortar is treated with strong boiling alcohol, on cooling white crystals are depo-

* From the *Annalen der Pharmacie,* Band xxix. Heft 2nd, Feb. 1839; and communicated by Mr. Richardson.

sited, which separate in larger quantity on further cooling. To this substance they gave the name of *amygdalin*. Liebig and Wöhler have determined this body to be an amide of amygdalic acid represented in the following formula:

$$N_2\,C_{40}\,H_{54}\,O_{22} + H_4\,O_2.$$

Subsequently the investigation was continued by Wöhler and Liebig, who observed that when a solution of amygdalin is brought into contact with milk of sweet almonds, a most remarkable and peculiar action takes place: prussic acid and oil of bitter almonds are formed, as in the instance already mentioned, when milk of bitter almonds is distilled without the artificial addition of amygdalin. Besides prussic acid and oil of bitter almonds there is also formed sugar, which may be decomposed by fermentation. The solution after the termination of the fermenting process affords a strong acid reaction, which is not produced by acetic or any other volatile acid. When alcohol is added and the solution is concentrated, thick white flocks are precipitated, which obviously contain no emulsin, because when dissolved in water they have no action upon amygdalin. From these and other properties the flocks would appear to be gum. The phænomena exhibited in the reaction described, which have been termed *catalytic* by Berzelius, resemble in great measure those which take place in fermentation, and their investigation promises to throw great light upon some of the most important processes of the vegetable and animal œconomy.

With the view of assisting in the elucidation of the subject we have commenced with the examination of the essential ingredients of the milk of sweet almonds which has been termed *emulsin*. The process by which we obtained the substance was as follows. Sweet almonds were triturated in a mortar, and small portions of water were gradually added until a milky fluid was obtained. This fluid was mixed with four times its volume of æther, and frequently agitated so as to effect an intimate mixture. A clear fluid gradually separated at the bottom of the stoppered bottle in which the experiment was made, and which at the end of three weeks was drawn off by means of a siphon. The fluid was passed through a filter, and to one half of the clear solution a large quantity of alcohol was added, which produced a copious deposition of white flocks which were *emulsin*. From the other half the emulsin was separated by bringing the solution to the boiling point, when it precipitated in flocky coagula. The emulsin precipitated by alcohol was carefully washed with the same fluid, and then dried over sulphuric acid in the vacuum of an air-pump to

avoid the effects of heat. In this state it possessed the following characters; it is a white powder, destitute of taste and smell, soluble in water, insoluble in alcohol and æther. When subjected to analysis in the usual way, the following results were obtained:

I. ·3485 grms. gave ·618 grms. CO_2 and ·2445 grms. HO.
II. ·3625 grms. „ ·6365 „ „ and ·2505 „ „

The relation of the carbon and azote was as 3 C to 1 A. From these results we have

	I.	II.
Carbon	49·025	48·555
Azote	18·910	18·742
Hydrogen......	7·788	7·677
Oxygen	24·272	25·026
	100·000	100·000

The fact of the existence of the substance operated on in the almonds appears to be established by its acting on amygdalin in the same manner as the milk of almonds alluded to in the commencement of the paper. After numerous trials with various reagents, its most distinguishing character was elicited by the phænomena exhibited when boiled with barytes. During the whole of the boiling, which was continued above six hours, ammonia was slowly and continuously disengaged. Through the solution a current of carbonic acid gas was passed, and the whole filtered; the clear solution was evaporated to dryness, and the residual salt, which contained a large quantity of barytes, possessed a strongly bitter taste; thus leading to the conclusion that *emulsin* is an amide; and that the salt formed by the action of barytes is a compound of barytes with an acid which we propose to term *emulsic* acid. The relations of *emulsin* to albumen, fibrin, and casein, &c. will form matter for subsequent investigation.

LXII. *On the Effects of Light and Air in restoring the faded Colours of the Raphael Tapestries. By Mr.* Trull; *communicated by* Michael Faraday, *Esq., D.C.L., F.R.S.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen, April 21, 1839.

YOU probably remember an exhibition in July last of certain Raphael Tapestries in the Haymarket, and the extraordinary effect the exposure to light and air had had in restoring and altering colours which had faded during cen-

turies of exclusion from these mighty agents. I have received letters from the proprietor Mr. Trull, and if you think parts of them worth publication at this time, when the action of light in the service of the fine arts is so much dwelt upon, they are entirely at your service.

I am, Gentlemen, yours, &c.,

April 21, 1839. MICHAEL FARADAY.

To Professor Faraday.

SIR, Warwick-row, Coventry, March 12, 1839.

The interest you took in observing the changes of colour in the Raphael Tapestries, after being exposed to light in London last July, made me anxious to communicate to you the extraordinary effects since produced, by the simple means suggested by yourself and other scientific gentlemen, of a more perfect exposure to light and air, which have for the last seven months been obtained, in a finely situated factory here.

I feared to trespass on your valuable time, but could not resist, after hearing of the great public interest now excited by the new process, called, I believe, "sun painting."

Light and air have done wonders for my tapestries, in dispelling the damp, clearing up the colours, and reproducing others, obscured by the effects of many years' close packing up in boxes. I regret not to be able to make scientific remarks on the progress of the recovery, which others acquainted with chemistry might have done.

The results cannot fully be appreciated but by those who recollect the work when up in London, where the first effects of change unexpectedly commenced.

The greens had all become blue; you, Sir, anticipated a return to the original tints, which has, almost throughout, taken place.

The robes and full colours generally had become dull and heavy; this has gradually gone off, and left a brilliancy of colour and beauty of effect hardly to be excelled. The gold also, as you hinted, has become more clear and bright.

The flesh parts of the figures, which had become pallid, almost to white, have recovered the high tint and deep shadow, and the strong anatomical effect of Raphael.

A renewed freshness now reigns over the whole, and the clearing up of the light in many of the landscape parts is most extraordinary, giving a depth and breadth the cartoons themselves do not *now* convey, particularly in the Keys to St. Peter, St. Paul at Athens, and The Death of Ananias;

where extensive landscape, ranges of buildings, and foliage have sprung up, like magic, on parts *quite obscured* when up in London eight months back, much of which is either worn, or torn out of Raphael's patterns at Hampton, and painted over, and known only through the means of these Leo Tapestries.

I should have much pleasure in giving you any further information on the subject I am capable of, or in showing the works to any persons taking an interest in them.

I am, &c. Wm. Trull.

Coventry, April 17, 1839.

My former letter in regard to the extraordinary changes the Raphael Tapestries had undergone the last seven months, use as you think proper. I regret not being able to give you a scientific description from the first, and the progress; and the absence of a gentleman acquainted with the chemical effects of light and air, to have noted the changes, is much to be regreted both for science and art. The works themselves being unique, and of above three centuries, so placed for so many years in continued damp to effect such mischief to the colours, are circumstances never to occur again.

Some colours entirely changed, others in confusion and apparently gone, yet by the *mere* effects of light and air, slowly and quietly resume the chief of their original tints! Flesh reappears, hair on the head starts up; the grand muscular effect and unique power of expression, only found in Raphael and Michael Angelo, are finely developed where a few months back appeared a plain surface! *Here* are the works, and the facts may be *now* ascertained.

I have applied to the directors of the British Institution, Pall Mall, to permit one or two of these Tapestries to be exhibited with the old masters in June; thus those who saw them last year, may be able to see what they now are, and both science and art may be served; for a comparison has never yet been made, since the Cartoons were repaired and painted upon, with the tapestry.

I think, Sir, you will recollect my subject of the stoning St. Stephen, the large masses of blue cloud-like appearance hanging about and over Jerusalem: these have nearly disappeared, and mountain scenery taken the place! The olive grove, which only showed a few trees in front all blue, and a heavy blue-like curtain was over all of the grove; the curtain has disappeared, and a fine deep grove is now seen; the natural green and mossy bank have nearly taken their original state; fresh lights keep breaking out and showing even deeper in the

grove! and throughout the works, the original lights are working their way, from the heavier colour.

I remain, Sir, yours, &c.,
WM. TRULL.

LXIII. *The Colours of the Atmosphere considered with reference to a previous Paper "On the Colour of Steam under certain circumstances."* By JAMES D. FORBES, *Esq., F. R. SS. L. & Ed., Professor of Natural Philosophy in the University of Edinburgh.**

IN the following Paper, it is proposed to illustrate more fully the hint explanatory of certain atmospheric colours, given in a notice of the remarkable red hue of condensing steam, communicated on the 21st January. Since that time, I have examined with care the principal authors who have adverted to the subject of the colour of the sky generally, and of the redness of sunset in particular; and since, in the course of that research, I have found much to confirm, and little to modify, the view which I have already taken of the subject, I hope that the present Paper may be considered as a fit appendix to my former experimental notice. It will be recollected that in it I stated the singular fact, that steam does not pass at once from the state of invisible pellucid vapour to that of a misty white cloud, such as issues from the spout of a tea-kettle; but that an intermediate stage occurs, in which it is coloured, even very highly, giving to transmitted light a hue varying from tawny yellow up to intense smoke-red. I then observed, that, since this phænomenon does not require steam of high tension for its production, it is very probable that the tints of sunset and of artificial lights seen through certain fogs, may be owing to the absorptive action of watery vapour in this critical condition.

Eberhard, a writer of more than sixty years ago, states that the multitude of opinions of authors on the colour of the sky alarmed him when he came to analyse them; and as, since his time, these have perhaps been doubled, some idea may be formed of the labour required to collect and classify the scattered notices which are to be found in special treatises, acade-

* From the Transactions of the Royal Society of Edinburgh, vol. xiv., part 2, p. 375; having been read before the Society on the 4th of February 1839. The previous Paper, read before the same body on the 21st of January, will be found in L. and E. Phil. Mag. for February, pres. vol. p. 121. A letter on the same subject addressed to Professor Forbes, by Mr. Webster, Sec. Inst. C. E., appeared in our number for March, p. 184.

mical collections, and periodical works, respecting it. The most copious references I have found amongst German authors, but these I have, in almost every case, been able to verify by a reference to the original authorities. The result has been a reduction to a few of those authors who have added anything of consequence to a subject which has rather been one of opinion than of science, until lately; and to still fewer of those who, by any one original observation or experiment, have added a single mite to the data for reasoning. The mass of copyists I may pass over in silence, or with little notice, and thus I hope to be able to reduce into moderate compass the results of a considerably tedious investigation.

It is impossible to advance any consistent theory about the colours of dawn, sunset, and clouds generally, without including the fact of the blue colour of the sky. The first notice I find quoted on the subject by way of explanation, is Leonardo da Vinci's *, who attributed it to the mixture of the white solar light reflected from the matter of the atmosphere, with the intense darkness of the celestial spaces beyond. This doctrine was also maintained by Fromond, and later by De la Hire, Funk, Wolff, and Musschenbroek, after the Newtonian theory of colours should have banished such reasonings from science. It was still later revived, to the disgrace of modern physics, amongst the chromatic fancies of Göthe †. Otto Guericke had nearly similar views.

The first trace of a more reasonable doctrine I find quoted from the writings of Honoratus Fabri ‡, probably from his Optical Essays, published at Lyons in 1667, and which must, therefore, have been independent of Newton's observations §. In opposition to the doctrines of Fromond, Fabri attributes the colour of the sky to the reflection of light, by corpuscular particles floating in the atmosphere; and Mariotte, about the same time, seems boldly to have maintained that the colour of air is blue ||.

Newton's thoughts on this subject are given, with his customary modesty, rather in the form of suggestions than asser-

* *Traité de la Peinture*, quoted in Gehler's Wörterbuch, art. *Atmosphäre.*

† Farbenlehre, i. 59, quoted by Humboldt.

‡ Eberhard in Rozier, i. 620.

§ Fabri's Dialogues (1669), of which I have found a copy in the Advocates' Library, contain many allusions to the imperfect transparency of the air, and the foreign particles mixed with it; but I do not find his theory of the blue colour clearly stated.

|| "On peut croire qu'il y a des couleurs primitives dans quelques corps, comme du bleu dans l'air. . . . . . Il semble qu'il y ait du verd dans l'eau."—*Mariotte, Œuvres*, i. 299. Leide 1717.

tions; and as many writers of the last century have only reproduced his ideas with slight alterations, it is important to observe his own exact statement of them. Newton's opinion respecting the colours of natural bodies, whatever judgment we may form as to its universal application, was singularly ingenious, and well worked out. He had discovered, in the course of his memorable investigation on the colours of thin plates, that every transparent body begins to reflect colours at a certain thickness; that these vary according to definite laws, as the thickness diminishes, passing through an immense variety of compound tints, until at length it becomes so thin (as in the case of the soap-bubble) as to be incapable of reflecting any colour at all: the last colour it reflects being orange, yellowish-white, and finally blue, before they vanish; these are called colours of the first order. Now, on this subject, Newton says, "The blue of the first order, though very faint and little, may possibly be the colour of some substances; and particularly the azure colour of the sky seems to be of this order. For all vapours, when they begin to condense and coagulate into small parcels, become first of that bigness whereby such an azure must be reflected before they can constitute clouds of other colours. And so this being the first colour which vapours begin to reflect, it ought to be the colour of the finest and most transparent skies in which vapours are not arrived to that grossness requisite to reflect other colours, as we find it by experience*." In another proposition, he says: "If we consider the various phænomena of the atmosphere, we may observe, that when vapours are first raised, they hinder not the transparency of the air, being divided into parts too small to cause any reflection in their superficies. But when, in order to compose drops of rain, they begin to coalesce and constitute globules of all intermediate sizes, those globules, when they become of a convenient size to reflect some colours, and transmit others, may constitute clouds of various colours, according to their sizes; and I see not what can be rationally conceived in so transparent a substance as water for the production of these colours, besides the various sizes of its fluid and globular parcels†."

The theory of Newton, therefore, embraces the colour of clouds, whether by reflected or transmitted light, as well as that of the blue sky. He applied a modification of the same theory to explain the *coronæ* round the sun and moon‡. The

* Optics, Book ii. Part iii. Prop. vii. † Ibid. Prop. v. end.
‡ Book ii. Part iv. Obs. 13.

air he seems to have believed to be devoid of colour, and the reflective particles to consist of vapour foreign to it.

The idea of Mariotte of the inherent quality of the sky to reflect blue light, was next prominently stated by Bouguer, who further put it in so palpable a form as to have been generally quoted since as a complete explanation of aërial colours*. He observes, that as red light penetrates further than blue (the reason is not mentioned), the latter is wholly reflected, whilst the former reaches the eye; and this theory was further improved by later writers, by ascribing superior *momentum* to the red rays, and inferior to the more refrangible ones. Smith, the author of the System of Optics, states the same view, but with greater clearness. "The blue colour of a clear sky," he says, "shows manifestly that the blue-making rays are more copiously reflected from *pure air* than those of any other colour; consequently they are less copiously transmitted through it among the rest that come from the sun, and so much the less as the tract of air through which they pass is the longer. Hence the common colour of the sun and moon is whitest in the meridian, and grows gradually more inclined to diluted yellow, orange, and red, as they descend lower; that is, as the rays are transmitted through a longer tract of air†;" and so he explains the colour of the moon in eclipses by the altered light refracted by the earth's atmosphere.

Next, Euler (1762) maintained the same opinion as to the blueness of the sky. "It is more probable," he says, "that all the particles of the air should have a faintly bluish cast, but so very faint as to be imperceptible, until presented in a prodigious mass, such as the whole extent of the atmosphere, than that this colour is to be ascribed to vapours floating in the air, which do not pertain to it. In fact, the purer the air is, and the more purged from exhalation, the brighter is the lustre of heaven's azure, which is a sufficient proof that we must look for the reason of it *in the nature of the proper particles of the air*‡."

The Abbé Nollet (1764) attributes the blue colour of the sky to its reflecting those rays; but, strangely enough, he supposes, that, in order to convey that tint to the eye, they must previously have come to the earth, been reflected by it, and stopped in their second transit through the atmosphere. The

* *Traité d'Optique*, p. 365–368. He likewise explains the coloured shadows noticed by Buffon.

† Smith's Optics, vol. ii. Remarks, 378.

‡ Euler's Letters (translation), ii. 507.

colour of the sun in a fog he attributes to the fog stopping the blue rays, at which time, he says, the atmosphere must appear blue externally to an observer in the moon *.

A very clever but little known writer, Mr. Thomas Melvill, who died in 1753, aged twenty-seven, has left some interesting observations exactly to our purpose, in a paper published in the second volume of the Edinburgh Physical and Literary Essays †. Amongst other acute remarks on optical subjects, after approving of Newton's theory of the blue colour of the sky, he objects to his explanation of the tints of sunset, justly inquiring, "Why the particles of the clouds become just at that particular time, and never at any other, of such magnitude as to separate these colours; and why they are rarely, if ever, seen tinctured with blue and green, as well as red, orange, and yellow?" "Much rather," he adds, "since the atmosphere reflects a greater quantity of the blue and violet rays than of the rest, the sun's light transmitted through it ought to draw towards orange-yellow or red, especially when it passes through the greatest tract of air; accordingly, every one must have remarked that the sun's horizontal light is sometimes so deeply tinctured, that objects directly illuminated by it appear of a high orange or even red; at that instant, is it any wonder that the colourless clouds reflect the same rays in a more bright and lively manner?" This he more fully illustrates, and then adds,—"Does it not greatly confirm this explication, that these coloured clouds immediately resume that dark leaden hue which they receive from the sky as soon as the sun's direct rays cease to strike upon them? For if their gaudy colours arose *like those of the soap-bubble*, from the particular size of their parts, they would preserve nearly the same colours, though much fainter when illuminated *only* by the atmosphere. About the time of sunset, or a little after, the lower part of the sky to some distance on each side from the place of his setting, seems to incline to a faint sea-green, by the mixture of his transmitted beams, which are then yellowish, with æthereal blue; at greater distances, this faint green gradually changes into a reddish-brown, because the sun's rays, by passing through more air, begin to incline to orange; and on the opposite side of the hemisphere, the colour of the horizontal sky inclines sensibly to purple, because his transmitted light, which mixes with the azure, by passing through a still greater length of air, becomes reddish." I have quoted this passage, because, so far as it goes, it ex-

* Nollet, *Leçons de Physique*, vi. 17. 1765.
† Page 81-89, &c. Edin. 1770.

plains with remarkable elegance the actually observed phænomena, and because it exposes the insufficiency of the theory of iridescent colours to explain the hues of sunset. The theory of vesicular vapour, or floating bubbles of water as constituting clouds, was prevalent even at a far earlier period than this. Leibnitz had supported it in the seventeenth century*, and had calculated the rarity of the æthereal fluid with which they were supposed to be filled. Kratzenstein (1740) had, by actual experiment on the colours which they reflected, attempted to estimate their thickness by direct measurement, to find their diameter†. Saussure demonstrated the existence of bodies apparently so constituted, in clouds themselves: but I nowhere find that he has applied it to explain their coloration on the principle which Melvill justly condemns in this passage. Saussure's opinion of the blue colour of the sky was, so far as I can judge, that of Mariotte and Bouguer‡, although he alludes very particularly to bluish vapours as foreign matters floating in the upper regions of the sky, which he says were decidedly not aqueous, since they did not affect the hygrometer§. He thinks this may illustrate the obscure phænomena of dry fogs||.

The memoir of Eberhard of Berlin on this subject¶ contains nothing to detain us. The author seems to coincide in the theory of Mariotte, and spends much labour in refuting that of Da Vinci.

Delaval's elaborate Theory of the Colour of Bodies, we may also rapidly dispose of. He adopts the idea of Fabri, that the foreign matters suspended in the air become the means of reflecting blue light, and transmitting red, on the same principle as arsenic dispersed through glass. This comparison to the acknowledged phænomena of opalescence, is not unimportant**.

The greater part of the optical writers of the present century have closely followed one or other of those already

* Opera Omnia, ii. p. ii. 82. Edit. 1768. "Cur vapores eleventur non spernenda quæstio est, atque inter alia non malè concipiuntur in illis bullæ insensibiles ex pellicula aquæ et aëre incluso constantes, quales sensus in liquoribus spumescentibus ostendit."

† Théorie de l'Elevation des Vapeurs et des Exhalaisons, &c. Bordeaux, 1740. Quoted in Saussure's Hygrometrie, § 202, and in Kämtz, Lehrbuch der Meteorologie, iii. 48. The diameter he made $\frac{1}{3000}$, and the thickness $\frac{1}{50000}$ inch.

‡ *Voyages dans les Alpes*, iv. § 2083.

§ Hygrometrie, § 355.

|| *Ibid.* § 372.

¶ Rozier, Introduction, i. 618.

** Manchester Memoirs, 1st Series, ii. 214, &c.

quoted. The writer of the article Optics in the 4th edition of the Encyclopædia Britannica, which was revised by Professor Robison, gives, as an opinion which he considers new, that of Bouguer and Melvill, with very little modification or addition. He assumes the greater momentum of the red ray (deduced, I presume, from the Newtonian theory of refraction), as the explanation of its greater transmissibility, and the reflection of the blue, attributing the colours of sunset to the former, those of a pure atmosphere to the latter. It would have been more correct, however, simply to assume the blueness of the atmosphere for reflected, and its redness for transmitted light, since we see in differently coloured media, that the assumed prerogative of the red ray does not hold, being absorbed by a green or blue glass, whilst the other rays persevere.

Humboldt gives no positive opinion upon the colours of the atmosphere, or of water*.

It is singular that I have been unable to discover in Dr. Young's various writings very positive notices of his opinion on this subject, though it is probable that he coincided in general with the view last stated†. He seems to have leaned strongly to Newton's theory of the colour of bodies, though he was not insensible to its difficulties.

Sir John Leslie very explicitly adopts the theory of air reflecting blue light, and transmitting orange, as a full and adequate solution of the colour of a pure sky, and also of the tints of yellow, orange, red, and crimson, which characterize the sun's light when near the horizon‡. The important observation of Sir D. Brewster ||, that the blue light of the sky is polarized, and therefore has undergone reflection, is conclusive on that point, although the cause of the peculiarities of the plane of polarization in different regions of the sky is not easily explained§.

Sir John Herschel coincides with Newton in considering the colour of the sky as the blue of the first order, and as one of the most satisfactory applications of the Newtonian theory¶.

But the author who, of all others I have met with, supports Bouguer's theory of the colour of the sky with greatest full-

* See his Rélation Historique, 8vo, ii. 116, &c.

† See his Nat. Phil. ii. 321. Compare pages 637, 638, 646, on Newton's Theory of the Colour of Bodies.

‡ Encyclopædia Britannica, art. *Meteorology*. The same theory is maintained in the article *Physical Geography* by Dr. Traill, just published.

|| On New Philosophical Instruments, p. 349.

§ Peclet, *Traité de Physique*, ii. 307. Brussels edit.; Herschel on Light, art. 858, and Quetelet's Supplement to the French translation.

¶ Essay on Light, art. 1143.

ness and ingenuity, is Brandes, in the article *Abendröthe* (evening redness), in Gehler's Physikalisches Wörterbuch *. He maintains the colour of the sun, and surrounding clouds, at sunset and sunrise, to be due *solely* to the colour of pure air,—a doctrine which he supports by many striking arguments. The presence of vapours, he observes, is always indicated by a dull white, mixed with the azure of the sky, and the complementary colour of that white which should belong to the transmitted ray can never be red. On the contrary, he says, the colour of the sun seen directly through clouds, when on the meridian, is always white, and the effect even of so strong a mist as to render his disc easily viewed by the naked eye, is to give it the appearance of a silver plate †. The beauty of the sunset, he further observes, is in exact proportion to the purity of the atmospheric blue during the day; and the only reason, he asserts, why the sun appears to set red through vapours, is because his light is by them so much diluted that the colour can be more distinctly perceived. The colour of elevated clouds, at some distance from the horizon, he imputes (as Melvill had done) to the great space of air which the light must traverse before it reaches them, and, after doing so, before it falls on the eye. The green colours of the sky he attributes, as Leslie and most other writers have done, to the reflected blue light mixing with the transmitted orange. This theory was never so ably handled.

[To be continued.]

LXIV. *On the Alluvia of Babylonia and Chaldæa.* By CHARLES T. BEKE, *Ph. D., F.S.A.*

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

IT is now two years since I last addressed you on the advance of the land at the head of the Persian Gulf ‡; I am now induced to revert to the subject by the recent publication of Mr. Ainsworth's "Researches in Assyria, Babylonia, and Chaldæa," (London, 1838), in which work are given the geological results of the Euphrates Expedition under the able conduct of Colonel Chesney.

Without going into the details of Mr. Ainsworth's investigations, it is sufficient to say that he determines "so far, the

* Vol. i. p. 4. &c. 1825.

† Gehler's Physikalisches Wörterbuch, vol. i. p. 6, Note.

‡ See Lond. and Edinb. Phil. Mag. for July, 1837; vol. xi. p. 66.

outline of the sea of Oman, or Gulf of Persia, at the earliest times of authentic history, [*i. e.* as appears from the context, at the time of Nearchus's voyage,] as to trace it from the territory of Ghoreïn, by Jebel Sinám or Teredon, Zobeïr, Spasinus Charax, and the Vallum Pasini in an undefined line across Dorákstán or Ká'bán, to the Arosis or Indíyán*;" and he also "finds evidence of the former existence of a lake, formed by the junction of the Tigris and the Choaspes or Eulæus, and extending from the neighbourhood of Diridotis to that of Aginis (Hawáz), and bounded to the north by the territory of Ampé, Aphlé, and Apamea, and to the south by that of Mesene and Charax†." His conclusion as to the advance of the land in this locality generally, is, that it may be calculated at about 30 yards *per annum*; which gives about 35 miles of increase since the time of Nearchus, and about double that quantity, or 70 miles, for the entire advance since the earliest post-diluvian periods ‡.

Hence it follows, as Mr. Ainsworth himself states §, that "Nearchus's fleet must have taken a diagonal or north-west course *across the country* which now forms part of Dorákstán and Mahersi;" a conclusion in direct opposition to the opinion entertained by Dr. Vincent, that "the pilot on board Nearchus's ship steered exactly the same course as McCluer's Karack pilot 2000 years afterwards ||;" and a sufficient confirmation of the correctness of the former of the two propositions advanced by me in my dispute with Mr. Carter, namely, "that, within the period of history, an advance of the land upon the sea has taken place of sufficient importance to affect *materially* the geography of the localities in question ¶,"—"and such, therefore, as to render the descriptions of ancient writers inapplicable to the present state of the country **."

The other of my propositions was, "that, within the same period of history, the advance of the land upon the sea has been so great as (independently of all other arguments,) to warrant my conclusion with respect to the non-identity of the Babylon of Nebuchadnezzar and the Babel of Genesis ††." On this point Mr. Ainsworth differs entirely from me; inasmuch as he makes the total advance of the land since the Flood only about 70 miles, whereas according to my hypothesis it must have been *at least four times as much.*

* See Lond. & Edinb. Phil. Mag., vol. xi. pp. 193, 194. † *Ibid.*
‡ See p. 194. § p. 187.
|| Commerce and Navigation of the Ancients in the Indian Ocean, vol. i. p. 466; and see Lond. and Edinb. Phil. Mag., vol. v. p. 248.
¶ L. & E. Phil. Mag., vol. viii. p. 506. ** *Ibid.* vol. ix. p. 42.
†† *Ibid.*, vol. viii. p. 506.

With all due deference to the opinions expressed by Mr. Ainsworth, and with a full consciousness of the great advantages which he has derived from an actual acquaintance with the localities in question, I will here merely raise one principal objection to the conclusion come to by him as to the advance of the land since the Noachian deluge being no more than 70 miles, from the circumstance that one of the main grounds for his thus restricting it, is the *assumption*, in the very outset of his arguments*, that the site of Babylon is actually identical with that of the tower of Babel; an assumption which leads him into difficulties not easily to be surmounted. I will briefly explain how this is.

Mr. Ainsworth states as the result of his observations, that "the last of the *deposits by transport* that is met with in that portion of the basin of the Euphrates which presents a rocky soil, is at a very short distance to the south of Hil †", and that "the limits of the *alluvia* are met with to the north in low hills and undulating land of tertiary rock formations, which advance to the banks of Euphrates at Mesjíd Sandabíyah, across the river about eight miles above Felújah or Anbár, and at the Pylæ of Xenophon rise in low hills above the plain of Babylonia, and towards Tigris are lost in the plains traversed by the Median wall ‡." This is, in fact, giving the tract of country *actually* formed by the alluvial deposits of the river almost the very same extent northward (*i. e.* above 70 miles to the northward of the ruins of Babylon,)as is *conjecturally* laid down in the map prefixed to my "Origines Biblicæ." Further, the western and eastern limits attributed by Mr. Ainsworth to these alluvia § apparently approach also very nearly to the limits given to them in my map ||."

* p. 104. † pp. 103, 104. ‡ pp. 112, 113. § p. 113.

|| The two severest and most detailed critiques of my *Origines Biblicæ* are from the pens of the well-known rationalist Dr. Paulus, in the *Heidelberger Jahrbücher* for January 1835 (new series, vol. ii. pp. 43—61), and the Rev. H. H. Milman in the Quarterly Review for November 1834 (vol. lii. pp. 496—519). Both these writers defend the *traditional* identification of Babylon with Babel! The latter says (p. 504), with reference to the above subject, "Mr. Beke's is the first attempt to reconstruct history on the principles of the young science of geology; but if historical speculation allies itself with science, it must submit to all the severe rules of scientific disquisition. It must take nothing for granted; it must not be contented with sketching on a map the probable line of coast which it may choose to assign to the Persian Gulf or any other body of water. It must not only enlarge, if necessary, the borders of the received chronology, but be in possession of accurate geological information as to the nature of the dry land which it thus converts into sea. When Mr. Lyell, or some other equally observant and highly gifted geologist, shall have sur-

Having then shown that "there occurs, at a rough calculation, a distance equal to, at the least, 70 miles, between the limits of the latest deposits by transport and the plain of Babylon*," Mr. Ainsworth proceeds to argue, that "as the site of Babylon is separated by a wide extent of deposits of a different nature from the latest deposits by transport which belong to the basin of the Euphrates, it is evident that it is impossible to reconcile the supposition of these latest deposits by transport being identical with the Noachian deluge, and of the deposits which intervene between them and the soil of the Tower of Babel, having been deposited in the short interval of time between the Deluge and the dispersion of mankind †."

He therefore concludes, that "the alluvium of the Euphrates divides itself distinctly into that which was ante-Babylonian (being also ante-Noachian) and that which is post-Babylonian; and the comparatively large extent of ante-Babylonian alluvium contains whatever matters the great cataclysm deposited upon the surface of the earth ‡."

But in coming to this result, Mr. Ainsworth does not appear to have considered a further conclusion which must necessarily follow from his premises. It is, that if the alluvial deposits of the basin of the Euphrates, consisting, as he informs us, of "clays remarkable for containing an excess of chloride of sodium or marine salt §," and extending from the neighbourhood of Felújah as far as the sea "to the exclusion of all other formations ||," be partly of *ante-* and partly of post-Noachian origin, then the Noachian deluge itself has, in point of fact, left no intervening traces whatever of its *separate* existence; and as such an event, let it have originated as it may **, could not possibly have happened without leaving traces, and those strongly marked ones, of its oc-

veyed the whole of this tract, and, on his geological responsibility, shall have, established we will not say, but, found reasonable grounds for conjecture, that at the date assumed by Mr. Beke the sea did advance so far inland, we shall bow to *his* authority."

It is therefore most gratifying to me to be able thus to appeal to Mr. Ainsworth as an authority for the fact that *the alluvial deposits do actually extend so far inland* as I had asserted. Their *date* is now the only point in question.

* p. 105.

† pp. 104, 105.—Mr. A. accordingly considers these latest deposits by transport to be the remains of a cataclysm anterior to the Noachian deluge. See p. 101. ‡ p. 107. § p. 105. || p. 106.

** Mr. Ainsworth cites without disapprobation my construction of the Hebrew words "fountains of the great deep," as meaning the "clouds;" whence I attribute the Flood to *rain* alone. See *Orig. Bibl.*, p. 319 *et seq.*

currence, to say that it has left no marks of its separate existence is tantamount to saying that it had no existence at all.

That this is not Mr. Ainsworth's intention is evident; and yet it is only a corollary inevitably resulting from his arguments. Hence I feel persuaded, that upon consideration he will admit that the whole of the alluvial deposits of Babylonia and Chaldæa, being *of the same formation*, without the intervention of the remains of any cataclysm, and proceeding from causes which are at the present day still in active operation at the mouth of the Euphrates, must necessarily have originated altogether since the Noachian deluge. If this be conceded, then Mr. Ainsworth's own arguments * go to prove that the distance of 70 miles between the commencement of the alluvia and the site of Babylon, could not have been formed in the period between the Deluge and the erection of the Tower of Babel, and consequently that the site of the former cannot possibly be identified with that of the latter; and when once this identification is done away with, and the simple historico-geological question is left free from the clog placed on it by the locality traditionally attributed to the Tower of Babel, I have every reason to expect that Mr. Ainsworth will see sufficient grounds for altering his opinion with respect to the *rate* of the advance in former days of the alluvial deposits upon the sea.

That all the historical evidences are in favour of separating the Babylon of Nebuchadnezzar from the Babel of Genesis, has been, I apprehend, already sufficiently shown by me in the pages of your Journal.

I should have wished to come here to a conclusion; but before doing so, I am, however unwillingly, compelled to advert to another subject.

Before proceeding on his Expedition to the Euphrates, Colonel Chesney was so kind as to assure me that the opinions expressed in my *Origines Biblicæ* with reference to the countries he was going to visit should receive every attention, and from the frequent reference to my writings made by Mr. Ainsworth, I conclude that if that gentleman was not already acquainted with my work it was placed in his hands by Colonel Chesney.

I will not think of complaining that in every instance in which Mr. Ainsworth has found it expedient to refer to me, with a single exception, that with respect to the cause of the Deluge, in p. 107, it has been only to differ from me in opinion †. Neither will I attempt the invidious task of seeking

* p. 105. † See pp. 53, 105, 126, 149, 160, 190.

how far the opinions enunciated by me might, not unreasonably, be presumed to have given rise to the expression of similar views, or to the consideration of the same subjects on the part of that gentleman: I am even willing to suppose that such coincidences as may exist are accidental. But I cannot do otherwise than express my regret that in one particular instance, in which he has done me the honour to adopt not merely my opinions but actually my words, reference should not have been made to the source from which they were derived. The passage in question, as given by us both, is as follows:

" In making the foregoing calculation [as to the comparative advance of the land in the Adriatic Sea and in the Persian Gulf,] it has been assumed that the circumstances are similar in both cases: this, however, is not precisely the case. The Adriatic is a gulf in a tideless (or almost tideless) sea: the rise and fall of the tide at the head of the Persian Gulf is (I believe,) as much as 8 or 9 feet at spring tides. From the effect, therefore, of the tide, and also from that of a current which sets across the head of the Persian Gulf from east to west, the accumulation at the mouths of the rivers would doubtless be checked, and a portion of the alluvium would be carried east [west]-ward and southward, and be dispersed in those directions over the bottom of the gulf. That such is actually the case is shown by the chart of this gulf lately constructed by the officers employed in its survey by the East India Company; from which it appears, that whilst along the north-eastern or Persian side of the gulf the

" In order to form even an approximative opinion upon the amount of time which the alluvial formations of Babylonia, Chaldæa, and Susiana have occupied in their deposition, all the various circumstances of their origin must be taken into account.

" The first and most important of which is, &c. &c.

" Fifthly, the nature of the waters which receive these alluvial deposits. The rise and fall of the tide at the head of the Persian Gulf is as much as nine or ten feet in spring-tides. There is, besides, a constant current which sets across the head of the gulf from east to west; the accumulations at the mouth of the rivers hence meet with a check, and a portion of the alluvium is carried to the westward and southward, and dispersed over the bottom of the Gulf; that such is actually the case, is shown by the chart of the gulf lately constructed by the officers employed in the survey by the Honourable the East India Company, from which it appears that whilst along the north-eastern or

depth, in great part, exceeds forty fathoms, along the whole of the Arabian or western and southern side it varies from twenty fathoms to shallows which are unnavigable, and which, to all appearance, will soon rise altogether above the level of the sea."—Phil. Mag. for July 1835, vol. vii. p. 43.	Persian side of the gulf, the depth, in great part, exceeds 40 fathoms, along the whole of the Arabian or north-western side, it varies from 16 fathoms to shallows which are unnavigable, and which to all appearances, will soon rise altogether above the level of the sea."—Ainsworth, p. 141.

I am ready to believe that the omission of the reference to me has been unintentional; still, in justice to myself, I am bound to allude to the fact.

I am, Gentlemen, yours, &c.,

Leipzig, March 23, 1839. CHARLES T. BEKE.

LXV. *Observations on the Anti-Inflammable and Anti-Dry-Rot Powers of the Subcarbonate of Soda and other Salts.* By HORATIO PRATER, *Esq.**

M. GAY-LUSSAC some years ago stated that if paper be dipped in a solution of phosphate of ammonia and dried, the *inflammability*† of such paper is destroyed.

I was induced by this observation in the winter of 1836 to prosecute this subject; and at that period, calico, wood and paper were kept immersed in various saline solutions for days together, in order to ascertain the comparative energy of such solutions in destroying the property of inflammability. As the object of these experiments was altogether practical, those saline solutions only were tried which could be obtained at a sufficiently low rate for general use. Accordingly, for the phosphate of ammonia proposed by M. Gay-Lussac, the muriate was substituted; and this was found to have the greatest effect in destroying the inflammable property of wood, calico, or paper. Wood should remain a week or ten days immersed in a saturated solution of it; for calico and linen twenty minutes, and for paper two or three hours at furthest is sufficient‡. If either of these be dried after such immer-

* Communicated by the Author.

† By this we mean that the paper cannot now be made to consume *with flame*. Held in a burning candle, it is still *immediately* carbonized and *gradually* dissipated.

‡ Some *very thick* paper was rendered uninflammable by immersion for six hours. Of course the time required in all these cases must vary with the thickness of the materials: but when we are sure that the solution has *thoroughly* permeated the texture, we should take it out, as this may be slightly injured by remaining exposed to the action too long. These observations in regard to the time of immersion apply equally to subcarbonate of soda.

sion, and then put into the flame of a candle, they turn black but do not take fire, and on being removed from the candle they do not continue to keep alight *like tinder*, ignited as it were, but without flame. Solution of muriate of tin possesses an anti-inflammable property to the same degree. These therefore will stand first on the list *.

It is worthy of remark, that neither calico, linen, paper, nor wood seem to lose their anti-inflammable property by process of time; at least I have some specimens in my possession, prepared in December 1836, which are still as inflammable as at first. This seems in accordance with Prof. Faraday's experiments, who did not find the muriate of ammonia to be volatilizable at the common temperatures of the atmosphere †.

But as neither the muriate of tin nor the muriate of ammonia is sufficiently cheap for *extensive* use, we are now to examine the fixed alkalies in reference to the property under consideration.

The subcarbonates of potass or soda seem sufficiently efficacious, though not to an equal degree with the salts first mentioned. There is little or no difference in the efficacy of either of these alkalies. They both prevent *inflammability*: but neither of them prevents *ignition* if we may so speak, that is to say, when paper or linen is prepared by them and held in the flame of a candle and then removed, no *flame* is communicated, but the ignited part or spark continues to spread *slowly* until the whole of the material is consumed. And this it does, whether the substance be held in one direction, or another; though of course the ignited margin extends most quickly when it is held in such a position that it can rise upward. It is to be observed, that whether calico, linen or paper be soaked twenty-four hours or a week ‡, in solutions of the alkaline subcarbonates, makes little or no difference in reference to this power of ignition. It is hence obvious, that the muriates of tin and ammonia are more decidedly anti-inflammables than the subcarbonates of potass or soda; but

[* Mr. Prater does not mention borax, which is one of the most effectual preservatives of muslin and similar fabrics from inflammation and rapid combustion.—Edit.]

† Phil. Trans., 1830. Some of the paper prepared by muriate of ammonia was kept by the author at a heat as great as the hand could bear (probably from 120° to 140°) for an hour, without being rendered in the least more inflammable.

‡ But the texture is injured by such long immersion, though not rendered more anti-inflammable, as many experiments have decided.

it seems not improbable that these latter may retain their powers longer*.

As there is little or no difference in the power of these alkalies, and as the latter is now very considerably cheaper than the former, we give it the decided preference.

For practical purposes, the subcarbonate of soda will, except in very particular cases, be found sufficiently anti-inflammable; for no great or *sudden* destruction of property which had been prepared by its solution could take place. Fire falling on one of the leaves of a book in a library so prepared, could scarcely be able to extend itself even through the book on which it fell; and certainly could not communicate to other volumes. And whether a child's dress, or the scenes of a theatre *so prepared* were set on fire, there would be little difficulty in extinguishing it. Although therefore the muriate of ammonia is a more complete anti-inflammable, its great expense compared to subcarbonate of soda is a formidable objection to its *general* use. Papers saturated with it might sometimes be used instead of parchment†, where it was the wish to give the *greatest* degree of security to the documents or productions.

In reference to *wood,* muriate of ammonia seems to have no advantage over the subcarbonate of soda. When wood, *although cut in the thinnest form,* is prepared by the solution of this alkali, the ignited part will not extend, as we have observed is the case with paper or linen under the same circumstances. The subcarbonate of soda, then, is what we recommend for the preparation of *all* articles composed of wood.

But it is fair to consider the grand objection to preparing wood by immersion in the saline solutions (for muriate of ammonia is equally liable to this objection with subcarbonate of soda). The objection alluded to is, that all these saline impregnations are completely removed by immersion in water‡,

* I have stated in the preceding page, that I have specimens of wood, paper, &c., prepared by the muriate of ammonia in December 1836, which are still quite uninflammable: the same is the case with those prepared by potass and soda at the same time. But it is still possible that these latter may remain uninflammable the longest. I was anxious to know whether the anti-inflammable power seemed to diminish by time; and as it has not hitherto, I hope these experiments will be more worthy public confidence.

† The reason why parchment is not so inflammable as paper probably is, on account of its containing more azote.

‡ It need scarcely be stated, that after the saline matter was removed, the wood *in all cases* was found to burn with flame, as usual.

or perhaps still more quickly by immersion in solution of soap and water. This was the case equally with muriate of tin, and some other solutions that were tried.

The objection, then, just mentioned will apply to wood that may necessarily be exposed to the rain, or which may require cleaning by soap and water*. This is the case with the deck of a ship and the floors of dwelling houses *as at present constructed.*

It is however to be remembered that even in these cases only *one* surface of the wood is exposed to such action: so that it seems very doubtful whether the combustibility could be more than very partially restored by such action of rain or soap and water. And on the deck of a ship the Anti-fire Preventive Company's composition †, or some coarse description of paint or varnish, might be used to prevent the contact of rain. The same remark applies to the floors of houses, except where the French mode of *dry*-cleaning and polishing could be adopted, or where carpets might be used.

But such seem the principal, or the only exceptions to the general advantage to be derived from the adoption of anti-inflammable wood. A great part of the wood used in building is placed *between* the floors, or on the sides of houses, which are usually painted. Of course in either of these cases wood prepared by subcarbonate of soda will retain its anti-inflammable properties unimpaired. And indeed with regard to the floors themselves, if after the manner of the French we paid more attention to the beauty of these than of our carpets, not having them except in the ordinary rooms so generally made of deal, wood prepared after the present suggestions could be still more extensively employed. Those who object to the

* This remark applies equally to *kyanized* wood. We have in many experiments found the corrosive sublimate to be removed by merely *immersing* such wood in water, or particularly *salt*-water, for a few days. Yet this fact seems quite overlooked by architects, and those concerned in rendering wood proof against the dry rot by Mr. Kyan's process.

† Lately patented. In this case it would not be necessary to use the composition for the *roof* or sides of the cabin. Great credit is due to the patentees of this invention for the liberal way in which they made their experiments before the public, but I should not be satisfied without further trial that the composition will *adhere* even to the walls, much less to the top of a room, when combustion to a certain extent is going on in the room. It appears to crack, and consequently to fall off, under the influence of a tolerably strong heat. If it be not influenced by wet, it would do probably for the *decks* of ships and for the *floors* of dwelling-houses and public buildings. However, it remains to be seen what effect much friction would have on it.

Since the above was written a second experiment has been made at Manchester, said to have been still more successful.

use of oak, beech, or any wood susceptible of the *dry* polish for their halls, staircases*, dining- and drawing-rooms, might still, by using *painted* deal under their carpets, employ uninflammable wood *even for floors*, with the certainty that (as water did not come in contact with them) they could not lose their powers of resisting flame by time. It is needless to say, that little expense would be incurred by such process, as the commonest sort of paint could, if desired, be employed for carpeted rooms; where oil skin was used, there would be no necessity for painting at all.

Of course, the preceding remarks, though applicable to all structures of wood, or partially of wood, are more particularly so to all offices and premises in which, from the trade pursued, or the number of documents kept in paper, the risk of fire is increased. And not only are they applicable to public and private buildings, but also to ships, and *particularly to steam-boats*.

Since this essay is intended for practical purposes, any *minute* inquiry into the *modus operandi* of the salts that have an anti-inflammable property, would be out of place. But as the carbonate (bicarbonate) of soda answers equally well as the subcarbonate (carbonate), it seems probable that these salts act in diminishing combustion, by the carbonic acid they contain. Muriate of ammonia probably acts by the ammonia. It is not perhaps so easy to form a plausible conjecture (for the above are nothing more) how muriate of tin acts, for neither bichloride of mercury (corrosive sublimate), sulphate of zinc, sulphate of copper, nor sulphate of iron, were found to have such property.

I shall close this paper by only a few more remarks, and first in reference to Mr. Benjamin Cook's patent for rendering wood, paper, &c. incombustible (to use his own term) taken out in 1822.

Mr. Cook used subcarbonate of potass, and likewise employed an instrument for extracting the sap. It appears pro-

* It seems *principally* by the staircases that fire communicates from one story to another. It is of immense consequence, therefore, for personal safety in case of fire, that these at least in *all* houses should be of uninflammable materials. Hence stone staircases are often very properly employed: but as these could not be used conveniently in *small* houses, any more than the more expensive sorts of wood, deal prepared by soda, and painted, ought certainly to be substituted, carpet or oil-cloth being laid down in the *middle* of the stairs. In this case, of course, as the paint *is never trodden upon*, it will remain as long without requiring to be renewed, as when employed for the sides of rooms; and consequently be of little or no expense compared to the improvement in the appearance of a staircase that it affects.

bable that the expense of Mr. Cook's process was the principal reason of its being little, if at all, patronized by the public. He was possibly also not aware that water extracted the alkali: if so, he cannot have given all the essential directions for the use of the invention. Besides, the extraction of the sap is an unnecessary and expensive operation, as *dried* wood does equally well for soaking in the solution. Nevertheless, Mr. Cook certainly has the credit, as far as my information goes, of starting this subject first. And the truth is (although it may appear unnecessary for me to state it), that I had made my experiments on the anti-inflammable properties of the alkaline carbonates and muriate of ammonia at Florence, many months before I was aware that a patent had ever been taken out on the subject, being principally urged to them, as already stated, by M. Gay-Lussac's observation.

But not only in the present inquiries do I find myself anticipated in some degree by Mr. Cook, but also by M. Durioz of Paris. This gentleman has taken out a patent in France, for rendering paper, and various articles of muslin, cotton, &c.*, uninflammable. He very judiciously recommends that ladies should have their dresses prepared by the process: we may add, that such a recommendation seems also very applicable to children, among whom instances of burning to death from the clothes catching fire are still more frequent than among adults.

M. Durioz has also especially recommended his invention to the notice of his government, for the purpose of rendering the scenes of theatres uninflammable; and the French Government, when I left Paris some months ago, was stated by the journals to have expressed themselves favourable to enforcing by law the use of M. Durioz's prepared scenes. It would be out of place on the present occasion to comment on such a law. If, as does not seem improbable, the slight additional expense of employing an effectual anti-inflammable preparation should operate with some stage proprietors or managers against its use, the justice and even benevolence of such a law would, it is presumed, be apparent.

As M. Durioz had not sent in his specification at the time I left Paris, I do not know the preparation he employs. As, however, I was informed that it was necessary to repeat the immersion each time the articles impregnated were washed, it is obvious that M. Durioz has not succeeded any better than myself in *fixing* the anti-inflammable preparation in the tissue, or substance operated on. This seems certainly a

* Wood, however, is not mentioned: I have reason to think, from this circumstance and others, that the process is not applicable to wood.

grand desideratum; but probably difficult, perhaps impossible. At all events, I shall confess that all my numerous experiments on the subject have been unsuccessful *.

It now only remains, in order to conclude this paper, to state, that it seems *probable* that subcarbonate of soda possesses, in addition to its anti-inflammable properties, a power of preventing the dry-rot. Mr. Cook, in reply to a letter I addressed to him on his patent, when I became acquainted with it, states that he has since discovered this to be the case. I am not aware that he has published any experiments on this subject; nor do I know whether his opinion be founded on experiments made by himself or others.

In the mean time I shall state the few experiments I made in support of such opinion, before I was aware that Mr. Cook considered the point as decided. They are not at present sufficient evidence of soda possessing an anti-dry-rot power; but I discontinued them after Mr. Cook's communication, conceiving he would not have made the assertion without sufficient evidence.

1. While a slight quantity of mould was visible on a piece of cream-cheese left for comparison, *none whatever* was to be seen on pieces of the *same* cheese that had been soaked sixteen hours either in solutions of bichloride of mercury, muriate of soda, muriate of tin, sulphate of copper, acetate of lead, muriate of ammonia, or *subcarbonate of soda.* Yet the observations were continued rather more than three weeks, viz. from December the 24th, 1838, to January 15th of the present year.

2. When about one-third of saturated solutions of bichloride of mercury and subcarbonate of soda was added to two-thirds milk in different glasses, in the former case the milk remained fluid † for three weeks, free from the slightest smell; in the latter a *very* slight, but not unpleasant smell was perceptible: *in neither was there any mould.* Yet some of the

* For the information of those females who do not wear dresses rendered uninflammable, it may be observed, that enveloping themselves in the hearth rug or counterpane is the best plan to put out flame if extensive.

† I have elsewhere observed (on the blood) that bichloride of mercury prevents the coagulation of this fluid, and have pointed out some analogies between the nature of this action in the blood, and the coagulation of milk *when rennet is added to it.* It seems in additional support of these analogies, that bichloride of mercury does not coagulate milk; and further, that it actually prevents its coagulation when disposed thereto by the acescent fermentation. I should have little doubt, that if bichloride of mercury were mixed with milk to which rennet had been added, no coagulation would take place.

same milk left for comparison in the same room was mouldy, having a strong cheesy smell, and being of course curdled.

3. Some pieces of meat, unboiled potato, and unboiled cabbage stalk, were put into water, and into a saturated solution of subcarbonate of soda at the same time, viz. in the beginning of January 1838. It is needless to say, that after a few weeks the animal and vegetable matter put into water began to grow putrescent; the piece of cabbage stalk and potato gradually breaking down and (so to speak) actually *dissolving* in the liquid; whereas those preserved in soda are even at the present time, April 1839, perfect in shape, and *almost as hard as when first put in.* It is astonishing that the piece of cabbage stalk should have remained so hard as it is.

It seems clear from these experiments that subcarbonate of soda prevents *two* kinds of decay or decomposition to which animal and vegetable matter is subject; first, that kind of decay which is attended with the growth of mould; and secondly, that kind of decay which proceeds further, and is synonymous with a complete dissolution of particles. Whether these be but degrees of each other, or be essentially different, we do not now inquire: it is sufficient for our purpose that the decay attended with the growth of mould is very analogous to that attended with the growth of fungus, or the rot (dry-rot, as it appears to be called, but perhaps improperly) of wood. It seems hence *extremely probable* that subcarbonate of soda which stops the one, would stop the other; but to establish this point with *certainty*, would of course require that experiments should proceed *for years* in a fungus pit,—as that of Woolwich. Nevertheless, the *extreme probability* that soda has an anti-dry-rot power must add to its value, though employed solely as an anti-inflammable.

LXVI. *Answer to the Objections published against a general Theory of the Visual Appearances which arise from the Contemplation of Coloured Objects.* By J. Plateau, *Professor at the University of Ghent.*

[Continued from p. 340, and concluded.]

SIR David Brewster also has honoured my theory of accidental colours, by attacking it (see L. & E. Phil. Mag., May 1834, page 353). I shall here quote the passage.

"The influence of strong light in rendering the retina partially insensible to red rays, even when these rays fall upon a part of the membrane which has not been directly acted upon by the strong light, has been finely shown by the ex-

periment of Dr. Smith of Fochabers *, and I have mentioned in a former paper † that a stick of red sealing-wax may be thus made to appear of a dark liver-brown colour.

"If we apply the strong light to the eye when the sensibility of the retina has been locally diminished by looking at a red object, a totally insensibility to red light will be produced. In order to observe this curious result in perfection, let the eye be steadily fixed for some time upon a seal of red wax which reflects white light from all its elevated parts. When the eye has been so fatigued that it would see a bright accidental green, bring a candle close to the excited eye‡, and so near its axis that the red seal will be seen by rays which pass near the flame of the candle. When this is done, the red wax seal will be converted apparently into a seal of black wax, the lights reflected from its elevations being still distinctly seen. This experiment, when successfully made, affords one of the most remarkable optical deceptions with which I am acquainted.

"The method now described of eliminating the impression of the primitive or exciting colour leads us to a very important determination in the theory of accidental colours. I endeavoured long ago to show from analogy, as well as from the evidence of experiment, that the vision of the primitive and the accidental colour is contemporaneous, in the same manner as the fundamental and the harmonic sound are heard contemporaneously by the ear. That this is the case may be shown in the following manner. When the eye is fatigued with the excitation of the red seal, a faint green phosphorescent-looking light covers for a while the surface of the red seal, occasionally overpassing its margin, showing, in the clearest manner, that the accidental green is seen at the same time with the exciting red. The effect of this vision of the green is to make the red appear much paler by its admixture with it. The red and green tend to produce whiteness; but as the direct red greatly predominates over the accidental green, the result is always a pale red. But when a brilliant light is brought near the excited eye so as to extinguish completely the red rays, the phosphorescent green appears alone; and thus we have ocular demonstration that the accidental green is not the light of a white ground deprived of the red rays to which the eye has been rendered insensible, but is a colorific impression generated in the retina itself, and superadded to the whiteness of the ground in the case when the

* Phil. Mag., vol. i. p. 250. † *Ibid.* p. 172.

‡ In this experiment, the other eye must be coloured.

eye is turned from the exciting colour to a white object. *These results are obviously incompatible with the theory of accidental colours recently published by M. Plateau.*"

This is a very abrupt conclusion, and the author will, I trust, permit me to consider how far it is grounded *.

Before we come to the experiment of the red seal, let us first examine this other fact related, as we have seen, by Sir David Brewster, namely, that during the contemplation of a red object, the colour of this object *appears to grow paler, or to mix itself up with a little white.* Certainly, if this fact was to be admitted in the full extent which the author attributes to it, its explanation in my theory would be difficult. But let us look into the matter more closely. In order to observe an accidental colour, the process frequently employed is to place the coloured object which must produce the phænomenon, upon a white ground. In this circumstance indeed, it seems that, by a prolonged contemplation, a little white mixes itself with the colour of that object; and it is, no doubt, in this manner that Sir David Brewster has operated. But, if we wish to notice the effect that a prolonged contemplation may have on the aspect of an object, is it not evident that the latter must be insulated from every extraneous influence, that it must be seen alone, or, in other terms, be placed upon a black ground? Now, in this case, the result is very different: the colour of the object, instead of growing paler, or blending itself with white, becomes, on the contrary, *darker*, or blends itself with black. The following facts leave nothing to doubt in that respect.

"If we look," says Buffon†, "for a length of time at a white spot upon a black ground, we shall see the white spot lose its colour." Now whiteness which loses its colour, what is it but whiteness which becomes dark?

Instead of the white object observed by Buffon, substitute one of any colour, for instance, a piece of red paper, placing it as before on a black ground; and, the better to judge the effect, employ an object of comparison. After having looked for a length of time at the red paper, the eye being invariably fixed on the same point, place close to this paper, and without altering the position of the eye, another piece of the same coloured paper. It is obvious that the image of this latter one

* I have answered that article of Sir David Brewster, above two years ago, in my detailed memoir, pages 29 and 56; but I deem it expedient now to renew this subject, with the intention of bringing together my answers to all the objections with which I have become acquainted. I shall besides consider the matter in a new point of view.

† Mem. of the Acad. of Sciences of Paris, 1743, p. 153.

falling on another part of the retina, the same may serve to establish a comparison, and allow you to appreciate the apparent alteration that the colour of the first object has undergone. Now this alteration is by this means rendered extremely sensible: the colour of the paper which has produced a prolonged impression, appears much darker than that of the other. It is therefore to be seen that we must attribute the effect observed by Sir David Brewster to the influence of the ground upon which the object is placed. I will quote, on that subject, a fact related by Scherffer: in describing a series of experiments, the view of which was to verify the effect pointed out by Buffon, and in which he looked at the object for a long time, he thus expresses himself (§ 16 of his memoir): "When I looked at white spots upon coloured paper, they seemed slightly tinted in the interior of their periphery with the colour of the ground. I would not, however, warrant that this effect always takes place." It appears then that from a prolonged contemplation there may arise, on the part of the retina occupied by the image of the object, an impression of the colour which surrounds that object. This can be ascertained by the following process, which renders the effect very intense. Place on a sheet of red paper exposed to a clear day-light, a small square of gray paper of such a tint as will appear neither paler nor darker than that of the sheet. Mark a black point in the middle of that small square, and keep your eyes fixed on this point for a sufficient length of time. You will shortly see on the square a feeble red hue manifest itself, which acquires more and more intenseness, according as the colour of the sheet appears to darken. Now, when a red object is placed on a white surface, the feeble tint of whiteness which seems to blend itself with the colour of that object, becomes a necessary consequence of the facts above mentioned. It remains to be shown in what manner these facts relate to my theory.

If accidental colours are owing to a reaction of the retina, it must be necessarily admitted that, during the contemplation of the object, a tendency to the reaction develops itself, that is to say, that the retina opposes to the action of coloured rays a certain progressive *resistance*; for we cannot conceive a reaction where there was no resistance. Now, this progressive resistance, or tendency more and more intense to constitute itself in the *opposite* state from which results the sensation of the accidental colour, must necessarily manifest itself by a gradual falling off in the apparent brightness of the object looked at. Thus, in the first place, is derived from my theory, as a necessary consequence, the fact that an object

insulated from every lateral influence, and contemplated for a long time, seems to become gradually darker.

During the prolonged contemplation of an insulated object, there are consequently two contrary forces in presence of each other, namely, the action of direct light, and the opposite effort of the retina. As long as the first force predominates, the eye feels the sensation of the direct colour, although more or less faded. But if by any cause whatever, the second force becomes overpowering, then the opposite sensation manifests itself, and the complementary colour is perceived. It is thus, for instance, that the experiment of the red seal is naturally explained: the strong light placed close to the eye paralyses the action of the red rays emanating from the seal, and the effort of reaction of the retina being then overpowering, the green accidental colour shows itself. In a certain point of view, as may be seen, Sir David Brewster's theory and mine approach each other; and indeed, in the first place, they have that in common, that they consider the accidental colour as owing to an impression of a peculiar nature, which is spontaneously generated in the organ, and not as the result of a relative insensibility to certain rays. On the other hand, according to Sir David Brewster, the accidental colour unfolds itself on the retina during the contemplation of the direct colour, and combines itself with this latter; and, according to me, the opposite effort of the retina, whence results the negative sensation as soon as that effort ceases to be counteracted, likewise unfolds itself during the contemplation of the direct colour, and combines itself in some respect with this latter, neutralizing it partially. But only, Sir David Brewster maintains that the combination of the two sensations produces whiteness, whereas I have shown that, upon an object insulated from every lateral influence, the result is on the contrary blackness.

At present it must be ascertained how my theory accounts for these lateral influences which tend to give the object a shade of the colour of the ground whereon it rests.

Let us primarily remark that, at the first sight, this fact seems in opposition with those composing the second section of which I have spoken at the beginning of this article, and constituting, on the retina, the passage to the normal state with regard to space. If, in fact, we bring together the experiments made on the phænomena of simultaneousness by Rumford*, Meusnier†, Prieur‡, &c., and the results ob-

* Philosophical Transactions, 1794.

† Meusnier's Experiments are related by Monge (*Ann. de Chimie*, tome iii. 1789.)

‡ *Annales de Chimie*, tome liv.

tained by M. Chevreul *, we shall come to this conclusion; that, whenever an object of small dimensions is placed upon a large coloured surface, the small object must take a tint more or less intense complementary to the colour of this surface †, and not one identical to that colour.

But there is an essential difference between the circumstances in which these two phænomena, apparently irreconcilable, are produced; for the one is the result of an *instantaneous* contemplation, and the other of a *prolonged* contemplation. In fact, by repeating the experiments which relate to the appearance on the small object of a tint complementary to that of the surrounding surface, it is easy to verify that this phænomenon shows itself in an instant with all its intensity; and this particularity had already been experienced by M. Chevreul (see the memoir quoted, page 41.). Now, what must happen, if the contemplation be prolonged? Since the complementary tint which covers the object results from the lateral action exercised on the retina by the impression of the surrounding colour, it is obvious that, if this latter impression becomes fainter, the complementary tint must undergo the same change. But the first effect being the necessary result of the length of the contemplation, the second is not a less inevitable consequence of it. Thus a prolonged contemplation will begin by weakening the impression of the complementary tint. And indeed I have verified this fact, (see my detailed memoir, page 33,) which can also be easily ascertained by others. But there is another cause which contributes to produce this degradation. To make it understood, let us take an example. Suppose the coloured surface to be red, and the small object black or gray. Then the prolonged contemplation will develop, on the part of the retina exposed to red rays, a contrary action; that is to say, an action tending to produce the corresponding *negative* sensation, or the accidental green; but the part of the organ which receives the image of the small object, and which is consequently alien to that negative action of the retina, must, according to the principles of my theory, exercise an effort in an opposite sense; that is to say, tending to give the corresponding *positive* sensation, namely a sensation of positive red. Now this

* *Mémoires de l'Institut*, tom. xi. 1832.

† It is only to be remarked, (as I have pointed out in the article of *Annales de Chim. et de Physique* which has been before alluded to, agreeably to the objections of the anonymous author) that in some particular circumstances the complementary colour does not extend itself on the whole surface of the small object, and the middle of this becomes then slightly tinted with the colour of the ground whereon the object is placed.

latter sensation being also contrary to the green negative tint which covered the small object at the first instant, there results from thence a new cause, which must concur with the former to weaken this negative tint. Much more, if the circumstances are such that the green negative tint be only faint, as it commonly happens when we look in a clear daylight, at a gray object placed on a red ground; then the above-named cause increasing continually, it must soon completely neutralize the green negative tint, and produce afterwards the apparition of a positive tint more and more intense. Thus is explained in the simplest manner the appearing, on the surface of the object, of a tint similar to that of the ground, and increasing in intenseness with the continuance of the contemplation. When the ground is white, the negative action of the corresponding part of the retina exercises itself at once upon all the rays which compose whiteness; and consequently in the interior of the space occupied by the image of the small object, the positive action of the organ must produce the sensation of all those rays combined together, that is to say, a sensation of whiteness. A small object placed on a white ground must therefore, by a sufficiently prolonged contemplation, appear to take a slight tint of whiteness. In the example before mentioned, I have supposed the object to be black or gray, to show the reasonings in a more simple light; but we conceive that they are also applicable to an object of any colour whatever.

I shall now quote new experiments which are very simple, and confirm the above explanation, in a manner that seems to me conclusive. They have been repeated with the same success by other persons.

1. Place on a black surface a sheet of coloured paper, for instance red, and in the middle of that sheet lay a small square of black or gray paper marked with a white point. Fix your sight on this point, till a red tint perfectly visible shows itself on the surface of the small square. Then, retaining this latter in the same place, withdraw suddenly the coloured sheet, so that the small square thus remains stationed on the black surface. At that very instant, you will see the red tint which covers it acquire a remarkable increase of intensity. In this case, in fact, the surrounding negative reaction of the retina operating freely, the positive reaction corresponding to the small square must also operate with freedom, and thus become much more intense.

2. Instead of withdrawing as above, the sheet of red paper, cast your eyes rapidly on the ceiling of the apartment, or cover them completely; then you will see a surface of an in-

tense green, having in the middle a red image equally intense corresponding to the small square. This fact evidently depends upon the same cause as the preceding one.

I shall terminate by a few words on the analogy admitted by Sir David Brewster between accidental colours and harmonic sounds. This philosopher considers as well as myself the accidental colours as an impression spontaneously generated in the organ. Now, it is admitted in physics that the harmonic sound has its origin in the sonorous body itself, which, besides the principal vibration, executes accessory ones. Moreover, the accidental impression continues after the disappearing of the direct impression, and nothing of the sort manifests itself with regard to harmonic sounds. The analogy in question appears to me therefore very remote.

The last objections that came to my knowledge have been raised by Mr. Osann, in the journal of Mr. Poggendorff*. But that article contains, with respect to my work, such inaccuracies, that they disfigure it and render it absurd: here is an example, of which the reader may judge upon good grounds. Mr. Osann describes in the following manner my experiment on the combination of two complementary accidental colours †:

"Place on a black ground a rectangle of paper, the halves of which are painted with two complementary colours, for instance red and green, and each one marked in the middle with a black point. *If you look for some time at this coloured rectangle, and afterwards shut your eyes completely, there will appear in its place a black image with a red point on one side and a green point on the other.*"

I think it needless to extend further the examination of this article, and to answer the objections therein contained.

LXVII. *On the polarized Condition of Platina Electrodes, and the Theory of secondary Piles.* *By* A Correspondent.

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

As so much interest has been lately excited by the peculiar *polarized* condition into which platina electrodes may be thrown, the following results, obtained a few months since,

* *Annalen der Physik und Chimie*, vol. xxxvii. 1836, p. 291, and following.

† See at the beginning of my present article, on the subject of the anonymous author's objections.

may perhaps be not uninteresting to some of your numerous readers. During a long series of experiments upon the theory of secondary piles, I had occasion to obtain and to verify the result announced by Mr. Golding Bird in the Philosophical Magazine for November 1838, namely, that the negative platina electrode of a voltaic arrangement which has been used in the decomposition of water, will give out less gas than the positive electrode under the influence of an equal negative current. I afterwards found,

1. That the *negative* platina electrode will in certain cases, and often most unexpectedly, give out *more* hydrogen than the *positive* electrode; that frequently when the two plates have been together active for some time (both giving out hydrogen) in a simple zinc and platina circuit, such as that described by Mr. Golding Bird, the (originally) negative electrode will evolve *least* gas in the first half of the experiment and *most* in the last.

2. That in this latter condition a delicate galvanometer will show the negative electrode to be *then negative* as it was before invariably *positive* to the other plate.

3. That this condition depends upon the relative cleanliness of the two electrodes. After scraping bright the surface of the negative electrode, or the one giving out least hydrogen in most cases, it gave off *still less* gas. If a similar operation were performed upon the positive electrode instead, the relative quantities of hydrogen evolved at the two plates approached *nearer to an equality*; finally, by using a negative electrode of which the surface was much discoloured, and a very bright positive electrode, I obtained the effect of *most* hydrogen evolved from the negative electrode, the latter being at the same time *actually* negative to the other plate. Thus, we may understand how it happens that the secondary current from two platina plates which have been repeatedly used to decompose water as negative and positive electrodes respectively, becomes gradually feebler and feebler, and at last altogether disappears.

4. That the secondary currents between the two electrodes is considerable in proportion as the water electrolysed by the plates has been pure, that is, as the matter evolved upon them has been purely *gaseous*. Thus a constant battery was connected during ten minutes with a decomposing apparatus in which stood the platina plates, and which was filled with a quantity of acidulated distilled water. When the connexion with the battery was broken the secondary current was 54°, estimated by the deflection of the galvanometer needle. A solution of sulphate of copper was then electrolysed for ten

minutes in the same way. At the end of that time a deposition of copper was formed upon the negative electrode. A secondary current of only 15° was obtained. This current varied in many experiments from 15° to 21°, as that obtained from the plates in the pure aqueous fluid from 50° to 54°. In general the secondary current in the sulphate of copper was only one third of that developed in the distilled water, after partial electrolysation. When a solution of nitrate of silver was exposed for ten minutes to the action of the battery, a secondary current varying from 9° to 12° was obtained. With a solution of sulphate of zinc (after the electrolysation of which a mixture of very impure oxide of zinc, and zinc appear upon the negative electrode) the secondary current was 20°. Even these currents, feeble as they were, I cannot help attributing solely to the gaseous combinations going on at those portions of the platina which remained exposed. The following experiment shows how very small a portion of the platina surface which has been active in the electrolysation of water is sufficient to the production of a far greater secondary current than any of those above mentioned.

The platina plate to be used as the negative electrode was divided into two portions, one of which was one eighth of the whole plate, which itself was not quite half an inch long and but the fifteenth of an inch in breadth. The two divided portions were so placed together during a few minutes' connexion with the battery as to form one electrode; connexion was broken; a secondary current of 54° was obtained; seven-eighths of the plate were then carefully and quickly removed; the current instantly fell to 30°; the removed portion was replaced; the needle was deflected 50°.

The current resulting from the *combination* of the gases oxygen and hydrogen upon the clear platina surface was often sufficient to *decompose water for some minutes.* Immediately that the connexion between the two plates was made this singular effect was produced, oxygen streaming up from the negative electrode and hydrogen from the positive.

5. The water electrolysed does not, as some have supposed, assist in the production of a secondary current by the oxygen which it holds in solution after electrolysation *independently* of that portion upon the surface of the positive electrode, as may be proved by changing the solution in which the plates have been active as electrodes of the battery, or by substituting for the positive electrode after electrolysation a fresh platina plate. In the latter case there is no secondary current. In the former the current will be the same, whether obtained through the partially electrolysed water,

or through a new alkaline or acid, or saturated saline solution.

I cannot help thinking that the theory of secondary piles, of which the functions seem to depend on chemical changes so very minute, will, when rightly understood, afford the best refutation of the doctrines of the *electromotists*. The beautiful experiments of Schœnbein and Matteucci have surely prepared the way for the establishment of such a theory!

I remain, Gentlemen, yours, &c.

J. B.

LXVIII. *Proceedings of Learned Societies.*

GEOLOGICAL SOCIETY.

President's Anniversary Address: continued from p. 387.

IN attempting a sketch of the subjects which have occupied the attention of the Society during the year, I should wish to retain that distribution of the science of geology according to which I arranged my remarks in the Address which I had last year the honour of reading to the Society; I mean the primary division into *Descriptive Geology* and *Geological Dynamics*; the former implying a description of the rocks of the earth's surface according to an established classification of strata and formations; and the latter dealing with the study of those general laws and causes of change by which we hope to understand and account for the facts which Descriptive Geology brings before us;—in short, the present condition and the past history of the earth's crust. But as the laws of permanence and change, with regard to organized beings, differ very widely from the dynamics of brute matter, we may conveniently make a separate study of the relations of organic life to which geology conducts us, and may mark it by the name *Palæontology*, by which it is commonly known. I will add, that it still appears to me convenient, for the present, to divide Descriptive Geology into two portions,—the Home circuit, in which the order of superposition has already been established with great continuity and detail; and the Foreign region, in which we are only just beginning to trace such an order. I shall also, as before, take the ascending order of strata. According to this arrangement of the science, I shall venture to bring to your recollection a few of the points to which our attention has mainly been called during the past year.

DESCRIPTIVE GEOLOGY.

1. *Home (North European) Geology.*—When I stated that Descriptive Geology has for its task the reference of the rocks of some portion of the earth's surface to an established classification into strata and formations, it was implied, that the more common employment of the descriptive geologist must be to refer the rocks which he examines to some classes *already fixed and recognized*; but it could hardly fail to occur to you, that from time to time the leaders

in this study will be called upon to execute a more weighty and elevated office, in framing the classifications which other observers are to apply; in drawing the great lines of division and subdivision which fix the form of the subject; in setting up the type with which examples are to be compared; in constructing the language in which others are to narrate their facts. Steps of this kind have formed, and must form, the great epochs in the progress of all sciences of classification, and especially in ours; and I need not remind you how great the importance and the influence of such steps amongst you have been. To pronounce at once upon the success of such steps must always be in some degree hazardous; since their success is in fact this, that they influence permanently and powerfully the researches, descriptions, and speculations of future writers; and there are few of us who can pretend to the foresight which might enable us to say, in any special case, how far this will be so. Yet the great works of Messrs. Murchison and Sedgwick, tending to the establishment of a classification of the strata below the old red sandstone (works which, on all accounts, we must consider as a joint undertaking), appear already to offer an augury which can hardly be doubtful, of this influence and permanence. Mr. Murchison's appellation of the "Silurian System" has already been adopted by MM. Elie de Beaumont and Dufresnoy, who have given it currency on the continent: M. Boué and M. de Verneuil announce the diffusion of "Silurian" rocks in Servia and the adjacent parts of Turkey in Europe; our own members, Mr. Hamilton and Mr. Strickland, have extended their range to the Thracian Bosphorus; M. Forchhammer, of Copenhagen, visited the "Silurian region" to endeavour to recognize the rocks of Scandinavia; and MM. Omalius D'Halloy and Dumont have just explored it, to establish a parallel between its deposits and those of Belgium. It will be observed that some of the districts thus mentioned are out of the limits of our geological Home circuit; and if the identification be really and permanently established in these cases, will extend the limits within which the parallelism of geological series can be asserted: and this is, in effect, what we have a right to look for, sooner or later, in the progress of geological science. As we must be careful not to apply our domestic types without modification to other regions, so must we take care not to despair of modifying our scheme, so that it shall be far more extensively applicable than it at first appeared to be. Of this progress of things examples are too obvious and too recent to require to be pointed out.

The labours of Professor Sedgwick refer to the "Cambrian System," which lies beneath the Silurian System, occupying much of North Wales, Cumberland, and a great part of Scotland; while the Silurian System spreads over a great part of South Wales and the adjoining English counties. The classification of the rocks of this portion of our island to which Professor Sedgwick has been led, though laid before you only at a recent meeting, is the fruit of the vigorous and obstinate struggles of many years, to mould into system a portion of geology which appeared almost too refractory for the philosopher's

hands; and which Professor Sedgwick grappled with the more resolutely, in proportion as others shrank away from the task perplexed and wearied. I need not attempt any detailed view of his system: his First Class of Primary Stratified Rocks occupies the Highlands of Scotland and the Hebrides, and appears in Anglesea and Caernarvonshire; the crystalline slates of Skiddaw Forest, and the Upper Skiddaw slate series come next. Above these is his Second Class, or Cambrian and Silurian System. The Cambrian is divided into Lower and Upper Cambrian, of which the former includes all the Welsh series under the Bala limestone; the two great groups of green roofing slate and porphyry on the north and south sides of the mineral axis of the Cumbrian mountains (of which groups the position had previously been misunderstood), and parts of Cornwall and South Devon. The Upper Cambrian System contains a large part of the Lammermuir chain; a part of the Cumbrian hills, commencing with the calcareous slates of Coniston and Windermere; the system of the Berwyns and South Wales; all the North Devon, and a part of the South Devon and Cornish series. Ascending thus through a series of formations distinguished and reduced to order by the indefatigable exertions and wide views of Professor Sedgwick, we arrive at the Silurian system; and here we must seek our subdivisions from the rich results of the labours of Mr. Murchison. These subdivisions were published in the summer of 1833. Like the Cambrian, the Silurian is divided into a Lower and an Upper System, the former including the Llandeilo flags and the Caradoc sandstones; the Upper Silurian Rocks being the Wenlock shale and limestone, the Lower Ludlow, the Aymestry limestone, and the Upper Ludlow, which finally conducts us to the Tilestones or bottom beds of the Old Red Sandstone.

That these various series of Cambrian and Silurian rocks are really superposed on one another; that they are justly separated into these groups; and that the smaller groups are truly of a subordinate nature, divided by lines less broad than those which bound the great series of formations;—these are points, of which the evidence must be sought in the works to which I refer. The evidence adduced by Prof. Sedgwick is mainly to be found in the great fact of superposition, supported by the circumstances of dip, strike, cleavage, mineral character, and all the great incidents of mountain masses. To proofs of this kind Mr. Murchison is able to add the testimony of organic fossils, of which a vast and most instructive collection is figured in his work. These fossils of the Silurian system, amounting in all to about 350 species, are essentially distinct from those of the Carboniferous System and Old Red Sandstone. This being so, the establishment of these great divisions is supported by that geological evidence which properly belongs to the subject.

In detecting order and system among the monuments of the most obscure and remote periods of the earth's history, it may easily be supposed that it has been necessary to employ and to improve all the best methods of geological investigation. Prof. Sedgwick's classification of the oldest rocks which form the surface of this

island has of course been obtained by a careful attention to the position and superposition of the mineral masses, and by tracing the geographical continuity of the strata, almost mile by mile, from Cape Wrath to the Land's End. In this manner he has connected the rocks of Scotland with those of Cumberland; these again with those of Wales; and the Welsh series, though more obscurely, with that of Devonshire and Cornwall. In this survey he has constantly kept before his eyes a distinction, known indeed before, but never before so carefully and systematically employed, between the slaty cleavage of rocks and their stratification; for the directions of these two planes, though each wonderfully persistent over large tracts, never, except by accident, coincide. He has taken for his main guide the direction of the strata, or, as it is called, the *strike* of the beds; and in such a course, the theory of Elie de Beaumont respecting the parallelism of contemporaneous elevations, whether true or false, could not fail to give an additional interest to geological researches, conducted on so large a scale as those of Prof. Sedgwick. Mr. Murchison's mode of investigation may be described thus: that he has applied, for the first time, to the rocks below the Old Red Sandstone, the method of classification previously employed with so much success for the Oolites. It is truly remarkable, that Nature has placed in this our corner of the world, series, probably the most complete which exist, of both these groups of strata; and as the Oolites of England have long been the type of that portion of European geology, the Silurians of Wales may perhaps soon be recognized as the standard members of a still more extensive range of deposits. As if Nature wished to imitate our geological maps, she has placed in the corner of Europe our island, containing an *Index Series* of European formations in full detail.

The Carboniferous, Old Red, Silurian and Cambrian systems have, by many writers, up to the present time, been all comprehended in the term "transition rocks", so far as that term has been used with any definite application at all. The analysis of this vague group into these distinct portions removes the confusion and perplexity which have hitherto prevailed in this province of geology. Prof. Sedgwick has further proposed to apply the term *Palæozoic*, and Mr. Murchison that of *Protozoic*, to the rocks which constitute the Cambrian and Silurian systems.

How far these appellations are useful, we shall see when we have had speculations presented to us in which they are familiarly used; for necessity is the best apology, and convenience the best rule, of innovations in scientific language. In the names applied to the members of the Silurian system, Mr. Murchison, following those examples of geological nomenclature which have been most clearly understood and most generally adopted, has borrowed his terms from localities in which standard types of each stratum occur. If the Silurian system be as exclusively diffused as some indications seem to imply, we may find the Ludlow Rocks in Scandinavia, and the Caradoc Sandstone even in Patagonia. Whether a like identification of the more ancient rocks of the Cambrian series with the

lowest formations of other countries be possible, may perhaps be (for the present) more doubtful.

I have spoken of Mr. Murchison's work as if it had formed part of our Proceedings, as indeed almost every part of it has done, although it now appears in a separate form. And I will add, that it is impossible not to look with pleasure upon the form in which the work appears, enriched as it is in the most liberal manner, with every illustration, map and section, picturesque view and well-marked fossil, which can aid in bringing vividly before the reader all the instructive and interesting features of the formations there described. The book must be looked upon as an admirable example of the sober and useful splendour which may grace a geological monograph.

Having been tempted to dwell so long on this subject from my conviction of its importance, I must the more rapidly proceed with the remainder of my survey. Mr. Bowman sent us, "Notes on a small patch of Silurian Rocks to the west of Abergele." In this investigation, which is interesting to us as the first application of Mr. Murchison's Silurian System, the author found strata of which some could be, by means of fossils, identified with the Ludlow rocks. Mr. Malcolmson has, by the remains of fossil fishes, shown that the calciferous conglomerate of Elgin represents the old red sandstone of Clashbinnie, as the Rev. G. Gordon had already supposed. Finally, proceeding to higher strata, we have to notice a trait of the fossil history of the coal strata near Bolton-le-Moors, contributed by Dr. Black. A stem of a tree thirty feet long, and inclined at an angle of 18° in a direction opposite to the strata, was discovered, having upon it a Sternbergia, about an inch in diameter, extending the whole length of the stem, which had been, while living, a parasite plant, like the mighty existing creepers of the tropical regions.

The most curious addition to our fossil characters of strata, are the footsteps discovered on the surface of beds of the new red sandstone. It is well known that several years ago such marks were discovered at Corncockle Muir, in Dumfries-shire. Since that time similar discoveries have been made at various places, and especially in 1834, in the quarries of Hesseberg near Hilbergshausen; and to the animal which had produced the impressions then discovered, the name of Chirotherium was provisionally applied by Professor Kaup. In the quarries of Storeton Hill, in the peninsula of Worrall, between the Mersey and the Dee, marks were discovered strongly resembling the footsteps of the Chirotherium of Kaup: these were described by a committee of the Natural History Society of Liverpool, and drawn by J. Cunningham, Esq. Mr. James Yates has also described footsteps of four other animals from the same quarries; and Sir Philip Egerton has given us a description of truly gigantic footsteps of the same kind, which he terms the *Chirotherium Herculis*.

Mr. Strickland gave us a notice of some remarkable dikes of calcareous grit which occur in the lias schist at Ethie in Ross-shire, and which had already been remarked by Mr. Murchison, in his

examination of the coast of Scotland, in 1826. They appear not to have been injected from below, but filled in from above.

Mr. Williamson's "View of the Distribution of Organic Remains in part of the Oolitic Series on the Coast of Yorkshire," was the welcome continuation of a labour of the same kind already executed for the lower portions of the series, and promised to be continued for the upper. Among the contributions to the fossil history of the oolites, we must also place Dr. Buckland's "Discovery of the fossil wing of an unknown Neuropterous Insect in the Stonesfield slate." This stratum, the Stonesfield slate, has, during the past years, occupied the Society in the consideration of its fossils in no small degree; but the speculations thus suggested belong to Palæontology rather than Descriptive Geology. Mr. Murchison's notice of a specimen of the Oar's rock, which stands in the sea off the coast of Sussex, nine miles south of Little Hampton, shows it to agree with some of the rocks in the greensand or Portland beds; and its thus belonging to the strata below the chalk falls in with the remark of its occurring between the parallels of disturbance which traverse the Wealden of Sussex on the north, and the Isle of Wight on the south; for these disturbances and other facts agree well with the notion of protruded strata between. The Wealden strata themselves have been observed by Mr. Malcolmson, at Linksfield, near Elgin. It is remarkable, that these strata had already, very unexpectedly, been found by Messrs. Murchison and Sedgwick in the Isle of Skye.

I have also to notice Dr. Buckland's account of the discovery of fossil fishes in the Bagshot Sands at Goldworth Hill, near Guildford. As these fossils resemble those of the London clay, Mr. Lyell's opinion that the Bagshot Sands were deposited during the eocene period is strongly confirmed.

The freshwater beds of the Isle of Wight, which had already supplied specimens of some of the Pachydermata of the Paris basin, have furnished an additional supply of rich fossils, which have been examined by Mr. Owen. He has found them to contain bones of four species of Palæotherium, and two species of Anoplotherium; also a jaw of the Chæropotamus, a fossil genus established by Cuvier; and another jaw closely resembling that of a Musk Deer, which Mr. Owen refers to the genus Dicobune, a genus also established by Cuvier upon the fossils of the Paris basin. Such discoveries, falling in with the conclusions obtained by the researches of previous philosophers respecting the tertiary period of the earth's history, and supplying what they left imperfect, cannot fail to give us great confidence in the results of those investigations, and to enhance our admiration of the sagacity which opened to us this path of discovery.

Dr. Mitchell gave an account of his attempts to trace the drift from the chalk and strata below the chalk, as it exists in the counties of Norfolk, Suffolk, Essex, Cambridge, Huntingdon, Bedford, Hertford, and Middlesex. This drift I had occasion to notice in my Address last year, in reference to Mr. Clarke's elaborate geological survey of Suffolk; and I then stated that this diluvial deposit is

known in the neighbourhood of Cambridge by the name of *brown clay*. Dr. Mitchell has shown that this deposit is of greater extent than we were before aware. But still to determine with precision its principal masses, total extent, and local modifications, would be a valuable service to the geology of the eastern part of our island.

As my order requires me to take the igneous after the sedimentary rocks, I must here notice Dr. Fleming's "Remarks on the Trap Rocks of Fife," which he distinguishes into three epochs;—those of the eastern extremity of the oolites, which are variously associated with the old red sandstone;—those which run from St. Andrew's to Stirling, which were produced after the coal-measures;—and those which occur along the shores of the Forth, which occur in the higher coal-measures.

2. *Foreign (South European and Trans-European) Geology.*—
In the survey of the progress of our labours which I offered to your notice last year, I stated, that in proceeding beyond the Alps, and I might have added the Pyrenees, we no longer find that multiplied series of strata, so remarkably continuous and similar, when their identity is properly traced, with which we have been familiar in our home circuit. Yet the investigations of Mr. Hamilton and Mr. Strickland appear to show, that we may recognise, even in Asia Minor, the great formations, occupying the lowest and highest positions of the series, which are well marked by fossils, namely the Silurian and Tertiary formations; and also an intermediate formation corresponding in general with the Secondary rocks of the north, but not as yet reduced to any parallelism with them in the order of its members. Besides these sedimentary rocks, in this as in most other countries, there are found vast collections of igneous rocks of various kinds, which interrupt and modify, and may mask and overwhelm, the fossiliferous strata. A paper has been communicated to us by Mr. Hamilton, "On a part of Asia Minor," namely, the country extending from the foot of Hassan Dagh to the great salt lake of Toozla, and thence eastwards to Cæsarea and Mount Argæus, and thus occupying a part of the ancient Cappadocia.

It appears that in this district the igneous rocks occupy a large portion of the surface, and the sedimentary strata which are associated with these are not easily identified with those which occur in countries already examined. The district examined by Mr. Hamilton contains a limestone belonging to the vast calcareous lacustrine formation of the central part of Asia Minor, and beneath this, a system of highly inclined beds of red sandstone, conglomerates and marls, which are perhaps connected with the saliferous deposits of Pontus and Galatia; but which could not be satisfactorily compared with the beds of the south of Europe, for want of the occurrence of organic remains. In only one instance did Mr. Hamilton observe the trace of organic bodies in the sandstone: these were impressions resembling fucoids, and similar to those found in the Alpine limestone near Trieste. Mr. Hamilton ascended to the summit of Mount Argæus, which had not previously been reached

by any traveller, which rises abruptly from the alluvial plain of Cæsarea to the height of 13,000 feet.

We have another contribution to the geology of the countries exterior to the Alps and Pyrenees in Mr. Sharpe's memoir on the geology of Portugal. He has examined with great care the neighbourhood of Lisbon, and has traced the superposition of the strata, naming the most conspicuous of them from the places in which they are well exhibited. His series (exclusive of igneous rocks) consists of San Pedro limestone (which rests upon the granite), slate clay and shale, Espichel limestone, red sandstone, hippurite limestone, a lower tertiary conglomerate, the Almada beds, and the upper tertiary sand. In the Memoirs of the Royal Academy of Sciences of Lisbon, for 1831, Baron Eschwege had examined a geological section taken across the mouth of the Tagus, and passing from the granite of the Serra of Cintra, to that of the Serra of Arrabida. But his identifications of the Portuguese beds do not agree with those of Mr. Sharpe, and have indeed the air of proceeding on the arbitrary assumption of a correspondence between this and other parts of Europe. Thus Baron Eschwege has referred both the San Pedro and the Espichel limestones to the magnesian limestone; the red sandstone formation he considers as Bunter Sandstein, while Mr. Sharpe refers it to the age of our Oolites: the hippurite limestone (now acknowledged to be the equivalent of our chalk and greensand) M. Eschwege makes to be Jura limestone; and the Almada beds he would have to be Plastic Clay and Calcaire Grossier. Mr. Sharpe is very properly attempting, by a further study of the organic fossils which he has procured, to confirm or correct the identifications to which he has been led. It is only by thus starting from different points, and tracing strata by their continuity, that we can hope to cover the map of Europe, and finally the world, with geological symbols of a meaning fully understood.

PALÆONTOLOGY.

The portion of our subject which we term Palæontology, might at first sight seem to form a part of zoology rather than of geology; since it is concerned about the forms and anatomy of animals, and differs from the usual studies of the zoologist only in seeking its materials in the strata of the earth's crust instead of upon its surface. Yet a moment's thought shows us how essential a part of our science the zoology of extinct animals is; for in order to learn the history of the revolutions which the earth has undergone, we must seek for general laws of succession in the remains of organic life which it presents, as well as in the position and structure of its brute masses. And since such general laws must necessarily be expressed in terms of zoology, it becomes our business to define those terms, so that they shall be capable of expressing truths which include in their circuit the past as well as the present animal and vegetable population of the world.

An example of this process has occupied a large portion of our attention during the past year. It appeared to be a proposition

universally true, that the oldest strata of the earth's surface contained cold-blooded animals only; and that creatures of the class mammalia only began to exist on the surface after the chalk strata had been deposited and elevated. And when, to a rule of this tempting generality, a seeming exception was brought under our notice, it became proper to examine, whether the anatomical line, which enables us to separate hot-blooded from cold-blooded animals, had really been rightly drawn; and whether, by rectifying the supposed characteristic distinction, the exception might not be eliminated. The exception on which this very instructive point was tried, consisted in a few jaw-bones of a fossil animal, which, though occurring in the Stonesfield slate near Oxford, a bed belonging to the oolite formation, had been referred by Cuvier to the genus Didelphys, and thus placed among marsupial mammals. In August last M. de Blainville stated to the Academy of Sciences of Paris his reasons for doubting the justice of the place thus assigned to the fossil animal. Founding his views principally upon the number and nature of the teeth of the fossil, he asserted that the animal, if a mammal, must come nearest the phocæ; but he rather inclined to believe it a saurian reptile; following, as he conceived, the analogies offered by a supposed fossil saurian described by Dr. Harlan of Philadelphia, and termed by him Basilosaurus. M. Valenciennes, on the other hand, asserted the propriety of the place assigned by Cuvier to the fossil animal, although he made it a new genus; and gave to the species the name *Thylacotherium Prevostii.* The controversy at Paris had its interest augmented when Dr. Buckland in September carried thither the specimens in question. From Paris the controversy was transferred hither in November, and principally occupied our attention at our meetings till the middle of January.

One advantage resulting from the ample discussion to which the question has thus been subjected, has been, that even those of us who were previously ignorant of the marks by which zoologists recognise such distinctions as were in this case in question, have been put fully in possession of the rules and the leading examples which apply to such cases. And hence it will not I trust be deemed presumptuous, if, without pretending to any power of deciding a question of zoology, I venture to state the result of these discussions. It appears, then, that some of the marks by which the under jaws of Mammals are distinguished from those of Saurians are the following: (1) a convex condyle; (2) a broad and generally elevated coronoid process, (3) rising near the condyle; (4) the jaw in one piece; (5) the teeth multicuspid, and (6) of varied forms, (7) with double fangs, (8) inserted in distinct sockets, but (9) loose and not anchylosed with the jaw. In all these respects the Saurians differ; having, for instance, instead of a simple jaw, one composed of six bones with peculiar forms and relations, and marked by Cuvier with distinct names; having the teeth with an expanded and simple fang, or anchylosed in a groove, and so on. Of course, it will be supposed, by any one acquainted with the usual character of natural groups,

that this line of distinction will not be quite sharp and unbroken, but that there will be apparent transgressions of the rule, while yet the unity of the group is indubitable. Thus the Indian Monitor and the Iguana, though Saurians, violate the *second* character, having an elevated coronoid process; but then it is narrow, and this seeming defect in our second character is further remedied by the third; for in those Saurians there is a depressed space between the condyle and the coronoid process quite different from that which a mammal jaw exhibits. Again, the teeth of Crocodiles, Plesiosaurs, and the like, are inserted in distinct sockets; but then they have not double fangs. The Basilosaurus was supposed to be a saurian with double-fanged teeth, but that exception was disposed of afterwards. And as there are thus saurians which trench upon the characters of mammals, there are mammals in which some of the above characters are wanting: thus the condyle is slightly or not at all convex in the Ruminantia; there is no elevated coronoid process in the Edentata; the Dolphin and Porpoise have not multicuspid teeth; the Armadillo has not varied forms of teeth, nor has it double fangs to its teeth, which also the fossil Megatherium has not. Still, upon the whole, the above appears to be the general line of distinction. Even if one or two of the above nine marks were wanting to prove the animal a mammal, still if the great majority of them were present, our judgment could not but be decided by the preponderance of characters. But if all the above characters of mammals are present, and all those of saurians absent, it seems to be a wanton scepticism to doubt that the animal was really warm-blooded.

Now it was asserted by Mr Owen, who brought this subject before us, that this is the case; that all the characters which I have enumerated above exist in the Stonesfield jaws. If we satisfy ourselves that this is the case, I do not see how we can avoid assenting to his opinion,—that the animal belonged to the class Mammalia.

Every such question of classification must resolve itself into two; that of the *value*, and that of the *existence* of the characters. If we assent to Mr. Owen in his view of the former, we are then led to consider the latter.

M. de Blainville, at least in his first examination, had laboured under the disadvantage of forming his judgments from casts and drawings only of the Stonesfield bones. Under these circumstances, he had denied several of the above characters; he had held that the teeth in the Thylacotherium are uniform; and that they are confluent with the jaw; and that the jaw is compound. These statements Mr. Owen, resting upon a careful examination of the specimens, contradicts. The assertion of the compound nature of the jaw is occasioned by a groove near the lower margin of the jaw, which however is not so situated as to represent the saurian sutures, but is completely explained by supposing it to be a vascular canal, such as exists in the Wombat, Didelphys, Opossum, and similar animals.

Another specimen, at that time the property of Mr. Broderip,

but now very properly placed in the British Museum, exhibits a jaw similar indeed to the Thylacothere, but belonging to a different genus; and to this species Mr. Owen has given the name *Phascolotherium Bucklandi.* Both these generic names imply that the animals are pouched animals; and in addition to the reasons which led Cuvier to this opinion, Mr. Owen has noticed in the fossils an inflection of the lower edge of the jaw, which, so far as has been hitherto observed, occurs in Marsupials, and in them alone.

As if this question had been destined to be settled at this time, the only remaining doubt with regard to the possible existence of double fangs in the teeth of a saurian was removed by the arrival in London of Dr. Harlan with his "Basilosaurus." That gentleman, with great liberality and candour, allowed sections of the fossil to be made in such a manner as to expose the structure of the teeth. And these being examined by Mr. Owen, and compared with the general laws of dental structure which he has lately discovered, it appeared that Dr. Harlan's fossil was by no means a saurian, but an animal nearly allied to the Dugong, to which Mr. Owen proposes to apply the generic name of Zeuglodon, expressing the conjoined form of its teeth.

I have not hesitated to lay before you the view of this subject to which I have been led by the discussions in which we have been engaged, notwithstanding the very great authorities which incline to the other side of the balance. Among these I hardly know whether I am to reckon Mr. Ogilby, who laid before us a very instructive communication, in which, without deciding the point, he pointed out the difficulties which appear to him to embarrass both views, and especially to contradict the opinion of the marsupial nature of the animal.

I have dwelt the longer on this controversy, since it involves considerations of the most comprehensive interest to geologists, and, we may add, of the most vital importance. For—*de summâ reipublicæ agitur*,—the battle was concerning the foundations of our philosophical constitution; concerning the validity of the great Cuvierian maxim,—that from the fragment of a bone we can reconstruct the skeleton of the animal. This doctrine of final causes in animal structures, as it is the guiding principle of the zoologist's reasonings, is the basis of the geologist's views of the organic history of the world; and, that destroyed, one half of his edifice crumbles into dust. If we cannot reason from the analogies of the existing, to the events of the past world, we have no foundation for our science; and you, Gentlemen, have all along been applying your vigorous talents, your persevering toil, your ardent aspirations, idly and in vain.

Besides the important investigations thus referred to, we owe to Mr. Owen other palæontological contributions. The genus *Chæropotamus*, established by Cuvier from an imperfect fragment of the bone of a skull, was asserted by him to be a Pachyderm most nearly allied to the Peccari. A fragment of a lower jaw of the same genus, found by Mr. Darwin Fox in the Isle of Wight, confirms this view, but indicates in some points an approach to the carnivorous type.

And it was remarked as interesting, that the living genus of the hog tribe which most resembles the Chæropotamus, the Peccari, exists in South America, where the Tapir, the nearest living analogue of the Anoplothere and Palæothere, the associates of the Chæropotamus, also occur. Another jaw, found by Mr. Pratt in the Binstead quarries in 1830, and resembling that of the Musk Deer, Mr. Owen refers to a new species of Cuvier's genus Dicobune, under the name *Dichobune cervinum.* Mr. Owen has also given us a description of Lord Cole's specimen of *Plesiosaurus macrocephalus*, which he compares with Mr. Conybeare's *Plesiosaurus Dolichodeirus*, by establishing an intermediate species, founded upon a specimen existing in the British Museum, and termed by him *Plesiosaurus Hawkinsii.* Besides tracing the analogies which connect these with each other, and comparing them with the two great modifications of the saurian tribe, the crocodiles and the lizards, Mr. Owen presented his remarks on the form of the Plesiosaurian vertebræ, founding them upon a general view of the elements of which all vertebræ are constituted.

To the communications thus made to us, we may add Mr. Owen's determination of another animal, of which the remains brought from the neighbourhood of Buenos Ayres, are among the many treasures of this kind which we owe to Sir Woodbine Parish. This animal, of gigantic dimensions, appears to have been allied to the Megatherium, but with closer affinities to the Armadillos; and it probably possessed the characteristic armour, of which, in the Megatherium, the existence is perhaps problematical. Mr. Owen has termed it *Glyptodon*, from the furrowed shape of its teeth.

In another communication Mr. Owen endeavoured to account for the dislocation of the tail of the Ichthyosaurus at a certain point, which is observable in many of the fossil skeletons of that animal. This circumstance, so remarkable from its general occurrence, and which Mr. Owen was the first to observe, he is disposed to account for by supposing a broad tegumentary fin to have been attached to the tail for a portion of its length, the position of which fin must, he conceives, have been vertical.

I cannot close my enumeration of the valuable contributions for which we are indebted to Mr. Owen, without remarking how well our anticipations have been verified, when, in awarding him the Wollaston medal last year, we considered the labours which we thus distinguished as only the beginning of an enlarged series of scientific successes; and how well also Mr. Owen's own declaration, that he should lose no available time or opportunity which could be applied to palæontological research, has been borne out by the services he has rendered that branch of our science.

In the remainder of my review of what has been done among us in Palæontology I must necessarily be very brief. I have already mentioned the discovery of fossil fishes in the Bagshot sand. These fishes have supplied three new genera, which Dr. Buckland has distinguished and has named *Edaphodon*, *Passalodon*, and *Ameibodon*; of which the two first offer combinations of the characters of bony and cartilaginous fishes. Mr. Stokes has given us his views of

the structure of the animal to which belonged those fossils with which we are so familiar under the name of Orthoceratites. He is of opinion, that these fossils, in their living condition, existed as a shell, enveloped within the body of the animal to which they belonged. He has distinguished three genera of these shells, to which he assigns the names *Actinoceras*, *Ormoceras*, and *Huronia*. The Marquis of Northampton also has examined those minute spiral shells which occur in the chalk and chalk flints, and have been termed Spirolinites. And, finally, under this head I must mention Mr. Alfred Smee's paper on the state in which animal matter is usually found in fossils.

Mr. Austen's hypothesis of the origin of the limestone of Devon, though belonging in some measure to Geological Dynamics, may perhaps be mentioned here, since he explains the position of those beds by reference to the habits of the coral animal. Mr. Austen has already shown himself to us as an excellent observer; and in constructing geological maps, a task requiring no ordinary talents and temper, he has earned our admiration. We shall therefore not be thought, I trust, to depreciate his labours if we receive with less confidence speculations in their nature more doubtful. As we can hardly suppose the calcareous beds of Devon to have had an origin different from those of other countries, we cannot help receiving with some suspicion a doctrine which would subvert almost the whole of our existing knowledge of the relations of fossiliferous beds of limestone.

LINNÆAN SOCIETY.

Dec. 4, 1838.—Read, "Observations on the Anatomical and Physiological Nature of Ergot in certain Grasses." By E. J. Queckett, Esq., F.L.S.

From the observations detailed in this paper, which have been followed up in many ergotized grasses, Mr. Queckett is inclined to believe that the ergot is a grain diseased by a particular parasitic fungus developing in or about it, whose sporidia find the young state of the grain a matrix suitable for their growth, and quickly run their race, not entirely depriving it of its vitality, but communicating to it such impressions, which pervert its regular growth, and likewise the healthy formation of its constituents, being at last composed of its diseased materials, which are mixed up with fungic matter, which has developed within it.

Dec. 18.—Read, "A notice of *Cereus tetragonus*," by Edward Rudge, Esq., F.R. & L.S.

Read, "Descriptions of the Indian species of *Iris*," by D. Don, Esq., Libr. L.S., Prof. Bot. King's College.

Read, "Additional observations on the *Spongilla fluviatilis*." By John Hogg, Esq., M.A., F.L.S.

Jan. 15, 1839.—Read, "A notice of the *Encephalartos horridus*, which flowered at Kinmel Park." By Mr. Thomas Forrest. Communicated by the Secretary; also "An account of the Indian species of *Juncus* and *Luzula*." By D. Don, Esq., Libr. L.S., Prof. Bot. King's College.

Feb. 5.—Read, a paper entitled "A Note upon the Anatomy of the Roots of *Ophrydeæ*." By John Lindley, Ph. D., F.R. and L.S., Prof. Bot. University College.

The object of the author in this paper was to show that salep, the prepared roots of certain *Ophrydeæ*, is not a substance consisting principally of starch, as is the common opinion among writers of the present day, but is composed of a bassorine-like matter, organized in a peculiar manner.

After stating the opinions of recent authorities, the author gives the results of his own microscopical examination of the tissue of recent and prepared roots, by which it appears that the tubercles of *Ophrydeæ* universally contain large cartilaginous nodules of a mucilaginous substance, not coloured by iodine, and a small quantity of the grains of starch, lying in the usual manner in the parenchyma which surround the nodules, and readily susceptible to the usual action of iodine. The tubercles of many South-African *Ophrydeæ* present when dried the appearance of bags filled with small pebbles, as if the epidermis had contracted over hard bodies in the inside. If a fresh root of *Satyrium pallidum* be divided transversely the cause of this appearance is explained, for with its soft parenchyma are mixed tough nodules, clear as water, and often twenty times as large as the cells which surround them. These nodules are easily separable, are tough like horn, and on being sliced appear to be perfectly homogeneous. They are scarcely soluble in cold water; when boiled they become tumid and partially dissolve into a transparent jelly. If exposed to the air they rapidly dry and become brown. The aqueous solution of iodine has no sensible effect upon them in their natural state.

On charring slices of some salep procured at Covent Garden, a coarse preparation of wild *Ophrydeæ*, the author found that the nodules apparently homogeneous were composed of extremely minute transparent cells, filled, as he supposed, with a secretion of the same refractive power as themselves, and adhering naturally to each other firmly; the double walls of the cells and intercellular spaces being only made apparent by the charring process. The author explains the error of those who have considered salep to consist chiefly of starch, by allusion to the mode of its preparation. The tubercles are first parboiled and then dried, the effect of which is to dissolve what starch exists in the cells surrounding the nodules. The dissolved starch flows over the surface of the nodules, from which when dried it is undistinguishable, and consequently when iodine is applied to salep the mass appears to become iodide of starch. If the nodules, however, after this action of iodine, be removed, they are seen to retain their original vitreous lustre.

The author remarks that these nodules of *Ophrydeæ* are, as far as his observations extend, absent in the tubercles of the other tribes of *Orchidaceæ*.

Read, a paper entitled "Some Data towards a Botanical Geography of New Holland." By Dr. John Lhotsky, late of the Civil Service, Van Diemen's Land. Communicated by Prof. Don, Libr. L.S.

Feb. 19.—Read, " Extracts from Letters addressed to Dr. Royle, V.P.R. & F.L.S., Prof. Mat. Med., King's College. By Dr. Falconer, Superintendent of the Hon. E. I. C.'s Garden, Saharunpore."

EDINBURGH SOCIETY OF ARTS.

April 17.—Among the communications laid before the Society at this meeting was the following:

Notice of recent improvements effected by him in Photographic Drawing, &c. whereby the lights and shadows are not reversed. By Andrew Fyfe, M.D., F.R.S.E., Vice-Pres. Soc. Arts.*

Before proceeding to describe his method of taking impressions without having a reverse, Dr. Fyfe stated that he had received communications from several gentlemen, mentioning that they had repeated his experiments with the phosphate of silver paper, and also with the ammonia, as a preservative. With regard to the latter, it was found by them all to prevent the further action of the light on the specimens, provided it was properly applied. As a test of its proper application, he stated, that the best method is to put the paper into a diluted solution of the ammonia, and to leave it for a short time, till all the yellow parts of the impression become white, showing that the whole of the yellow phosphate is washed out. An impression on paper, taken by Mr. William Forrester, lithographer, was shown. In this the lights and shadows were preserved as in the original, by covering the light parts of the drawing from which it was taken with a dark ground, and leaving the darker parts lighter and lighter, so as to allow the greater transmission of light through what in the original was the darkest. A lithographic stone was likewise shown, on which an impression was taken by Mr. Nichol, lithographer, by covering the stone with phosphate of silver, and then, after putting an engraving on it, exposing it to light in the usual way. Dr. Fyfe then proceeded to describe the process by which he had succeeded in getting impressions, in which the lights and shadows are not reversed. For this purpose the phosphate paper is first darkened by the action of light; it is then immersed in a solution of the iodide of potassium, and while still moist, exposed to light, with the object, the impression of which is to be taken, placed on it, and left till the whole of the paper exposed becomes yellow, and when removed it exhibits a distinct representation of the object. In this process there is a tendency of the iodide to convert the dark phosphate into yellow iodide of silver, which it does instantly when the solution is strong, but very slowly when it is weak, unless it is exposed to light, and then the action goes on rapidly. It was observing this that induced Dr. Fyfe to try the influence of light on phosphate paper besmeared with iodide of potassium, by which he was led to the discovery. Of course, when an object, which allows the light to pass through it differently, is put on the paper, those parts on

* For other papers on this subject see our numbers for March and May, p. 196, 365, and 368: also Miscellaneous Articles in the present number.—EDIT.

which the denser portions of the object are placed still retain their dark colour, the other parts are tinged, just according to the transmission of the light. When impressions thus prepared are kept, they gradually begin to fade, owing to the continued action of the iodide of potassium, and hence the necessity of submitting them to a preservative process. After numerous trials, that which seemed to answer best, was merely immersing them in water for a few minutes, and in some cases even allowing a stream of water to flow gently on them, so as to wash out the whole of the iodide of potassium not acted on—in this way the agent which tends to decolorate the blackened phosphate, seems to be removed. Several specimens of impressions from dried leaves were shown, in which the different parts seemed to be as nicely delineated, as in the other process, where the reverse is given. Dr. Fyfe, however, stated, that though they seemed to be preserved, yet he could not positively assert that they were so, as they had been prepared only a few days. He also mentioned, that owing to the unfavourable state of the weather, he had tried the Camera Obscura only once, and though there was no sunshine, he had succeeded in getting a very faint delineation, from which he was in hopes that, in this way, a true representation may be obtained. He also stated, that impressions may be got by using darkened chloride paper in the same way. In this case the solution of iodide must be much weaker than that for the phosphate, the chloride being more easily acted on. In both cases the solution is made of such strength, that when the paper is touched with it, it acts on the phosphate or chloride feebly, and then, before being spread on, it must be diluted with a little water, so as just to have it of such strength that it does not act. A specimen of impression from chloride was also shown.

LXIX. *Notices respecting New Books.*

An Elementary Treatise on the Tides. By J. W. Lubbock, Esq., Treas. R.S., F.R.A.S., and F.L.S., Vice-Chancellor of the University of London. Lond. 1839, pp. 54; with two plates of tables and a map.

We find in the Preface to this work a notice of the origination, in the hands of Mr. Lubbock, of the attention which the neglected subject of the tides has of late years received from British men of science, and of which our reports of the proceedings, both of the Royal Society and of the British Association, nearly throughout the present series of the Philosophical Magazine, have presented the main results, chiefly as given in the researches of Mr. Lubbock and Prof. Whewell. Mr. Lubbock's first examination of the London Dock observations was undertaken in 1829, at the instance of the Committee of the Society for the Diffusion of Useful Knowledge, with the view of obtaining correct tables for predicting the time and height of high-water for the British Almanac. When, however, the quantity which is called the *semi-menstrual inequality* had been determined with sufficient accuracy for practical purposes, the further researches in the difficult problem, the solution of which had thus been essayed, could

not have been so soon undertaken, but for the interest felt in the subject by some individuals distinguished in science, particularly by Mr. Whewell, and but for the pecuniary grants which were in consequence devoted to it by the British Association.

Mr. Lubbock having been thus enabled to procure the valuable assistance of Mr. E. Russell and Mr. Jones, the results have been published by him in the Philosophical Transactions, as above intimated; "but as he is not aware," he remarks, "that any detached elementary Treatise on the Tides exists in the English or in any other language," he trusts that a more connected view of the subject may not be uninteresting. In the interest of the connected view of the subject which Mr. Lubbock has taken, accordingly, in the work now before us, every one must concur who is enabled to appreciate its importance.

The treatise commences with an historical notice of the inquiry into the theory and laws of tidal phænomena, tracing it from Pliny to Kepler and Wallis; stating in succession the true theory as discovered by Newton, and the researches of Bernoulli and Laplace, and concluding with the remark, "The attention of Laplace does not appear to have been directed to the construction of Tide Tables for predicting the time and height of high-water at any port; until very recently, this practical solution of the problem was attempted only in Great Britain."

We next have the "Mathematical Theory of the Oscillations of the Surface of the Ocean;" which is succeeded by the "Comparison of Theory with Observation," the "Method of predicting the Time and Height of High-water," and copious and explicit directions for making and recording "Observations of the Tides."

We extract the following remarks on the relations of the subject of the tides to various other branches of science which occur incidentally, but which we conceive are of great interest.

"The branch of the subject which appears at present to stand in need of the most laborious exertions, is the accurate determination of the *semi-menstrual inequality* and the local constants (from which the quantities (A), D, (E) may be immediately obtained) for other places than Liverpool and London. Until this is done, our knowledge of the progress of the tide-wave and of the circumstances which attend it must be very defective. This question is not uninteresting to geologists, for, until it has been taken up carefully and extensively, it will be impossible to detect slight changes in the relative level of sea and land, except in narrow seas, such as the Baltic, which are exempt from tides. Even to trace the gradual extinction of the *semi-menstrual inequality* between London and Teddington, or in any similar locality, would not be unprofitable."—Pref. p. v.

"The great real obstacle to perfection in calculations or *predictions* of the tides consists in the fluctuations of the *establishment.* Suppose the establishment (or the constant to be added to ψ) changes a minute *per annum*, and that having determined it from all the observations of twenty-one years, we proceed to employ it in calculations of the time of high water. It is obvious that if we calculate

for the *eleventh* year, and compare the calculated times of high water with the observed times, they will not be affected with any *constant* error, but if we calculate for the *twenty-first* year the calculated times will have a constant error of 10 minutes. Similar remarks apply to the heights, that is, to the constant D. If the channel becomes deeper, the tide-wave travels with greater velocity, and the high water happens sooner. Thus again an indication arises which may hereafter be useful to the geologist."—Ib. p. vi.

"The practice which at present obtains of referring the heights of buildings and mountains to the *level of the sea* or to *high or low water mark*, seems objectionable. The heights of *spring* or *neap* tides, although not subject to so much uncertainty, are also quantities too vague to be used with propriety as standards of reference." —p. 24.

"The *establishment* of London (*i. e.* the interval between moon's transit F at $0^h 0^m$ and the time of high water) appears now to be very different from what it was in the time of Flamsteed. It seems to have fluctuated more than ten minutes even since the commencement of this century. At the London Docks, in 1807, it was $2^h 0{\cdot}9^m$; in 1818, $1^h 57{\cdot}0^m$; in 1835, $2^h 4{\cdot}4^m$.—See Phil. Trans. 1837, p.136, and the accompanying plate. These numbers were obtained by Mr. E. Russell, and the corrections requisite in order that they might refer to the same parallax and declinations were carefully applied.

"This perplexing fluctuation presents an insuperable obstacle to extreme accuracy in tide predictions, until it can be explained. It is probably owing to changes in the bed of the river, the drainage of the banks, &c., which it is impossible to embrace in any mathematical formula. Perhaps the manner of taking the observations may have varied slightly.

"I am indebted to Mr. Yates for notice of a very ancient tide table which exists in a MS. in the British Museum. It is in the Codex Cottonianus, Julius DVII., which appears to have been written in the 13th century, and to have belonged to St. Alban's Abbey. It contains calendar and other astronomical or geographical matters, some of which are the productions of John Wallingford, who died Abbot of St. Albans, A.D. 1213. At p. 45 b. is a table on one leaf, showing the time of high water at London Bridge, 'flod at london brigge', thus:

Ætas
Lunæ.

	h	m
1	3	48
2	4	36
3	5	24
4	6	12
....	....	....
....	....	....
28	1	24
29	2	12
30	3	0

N.B. The numbers increase by a constant difference of forty-eight minutes. The first column gives the moon's age in days.

Hence it would appear that high water at London on full and change was at that epoch $3^h\ 48^m$, or more than an hour later than at present. The time of high water at London on full and change is given in Mr. Riddle's *Navigation* and in other works $2^h\ 45^m$; Flamsteed made it 3^h."—pp. 32, 33.

"It has been remarked, that in consequence of the sheltered situation of the port of London, the great undulations produced by the winds will be less sensible there than on the coasts of France, as at Brest for example. But it should be recollected, that if the tide at London is to be considered as a *derived* tide, transmitted from the Atlantic, the irregularities which are felt at Brest will equally tend to affect it at all those places which it reaches subsequently.

"'During strong north-westerly gales, the tide marks high water earlier in the river Thames than otherwise, and does not give so much water, whilst the ebb-tide runs out later, and marks lower; but, upon the gales abating and the weather moderating, the tides put in and rise much higher, whilst they also run longer before high water is marked, and with more velocity of current, nor do they run out so long or so low.' For this information with respect to the influence of the wind on the tides in the river Thames, I am indebted to Sir J. Hall.

"It has been found, that since the construction of the new London Bridge and the removal of the old foundations, there is less water at the St. Katherine Docks at low water by about 18 inches than formerly, but as respects the depth of high water it is the same; in other words, the flood-tide at the entrance of the St. Katherine Docks lifts about 18 inches more within the time of flood than formerly. I am indebted to Sir J. Hall for this information. I do not however attribute the fluctuations of the *establishment* to the removal of the old bridge. They began long before the foundations were touched."—pp. 48, 49.

We hope that the students of exact science may receive many more elementary treatises from the pen of the same distinguished author.

Geometrical Theorems and Analytical Formulæ, with their application to the solution of certain geodetical problems, with an Appendix containing a description of two copying instruments. By WILLIAM WALLACE, LL.D., Emeritus Professor of Mathematics in the University of Edinburgh; Fellow of the Royal Society of Edinburgh; Fellow of the Royal Astronomical Society; Member of the Cambridge Philosophical Society; Honorary Member of the Society of Civil Engineers, &c. 155 pp. 8vo.

THE above work is the production of a man who has been well known to the scientific world for nearly fifty years, and who, in the course of that period, has distinguished himself by excellent treatises on various branches of mathematics in the Encyclopedias, and by original mathematical papers of great merit in the transactions of learned societies. It has been written in interesting circumstances. Several years ago he retired from the active duties of

his chair, in consequence of infirm health, and the present work is the fruit of his leisure, indicating however that, while the powers of his body have been enfeebled, the powers of his mind remain unimpaired. It contains many new, curious, and elegant properties of triangles, quadrilaterals, and conic sections. It abounds also with practical applications, particularly to surveying on an extensive scale. One of the most useful problems in surveying is, *given three points in a country, and the angles which the distances between them subtend at a fourth, to find the distances of these three from the fourth.* It is a problem as old as the days of Hipparchus, was applied by Suellius in measuring a degree of the meridian, and has engaged the attention of distinguished analysts in more recent times. This problem Mr. Wallace has solved in a manner extremely convenient for the application of logarithms. Another problem to which Mr. Wallace has paid great attention in this work, may be enunciated as follows: *four points in a country being supposed to be joined by straight lines, given the angles of two of the triangles having one of the six straight lines for a common base, and given also one of the six lines, to find all the other lines and angles in the figure.*

He has shown the success of his formulæ by application to examples taken from the great trigonometrical surveys of Britain and France.

The value of Mr. Wallace's work to surveyors is greatly increased by a particular description of an instrument which he invented some years ago, and which he calls an *Eidograph.* The object of it is to reduce or enlarge plans in a given ratio. This it accomplishes in a manner far superior to the *pantograph,* the only instrument previously known for that purpose. Mr. Wallace described the eidograph in the 13th vol. of the *Transactions of the Royal Society of Edinburgh.* But it may be hoped that the present work will make this valuable contribution to the arts more extensively known. He has also given a description of some other instruments which he has invented for the solution, by geometrical construction, of the first of the two problems mentioned above.

Mr. Wallace's work assumes particular importance at the present moment, when the trigonometrical survey of Britain, which had been suspended for several years, is about to be resumed. We hope that the author's health will continue such for many years as to enable him to enlighten the public by his ingenious labours.

The Eidograph is made by Mr. Adie, optician, Edinburgh. Price nine guineas.

Experimental Researches in Electricity. By Michael Faraday, D.C.L., F.R.S., Fullerian Professor of Chemistry in the Royal Institution, &c. *Reprinted from the* Philosophical Transactions of 1831–1838. Lond. 1839. 8vo. Pp. 574, with Eight Quarto Plates.

It is unnecessary, in addressing the readers of the Philosophical Magazine, either in that particular character, or as members of the

republic of science in general, to expatiate on the subject of Professor Faraday's discoveries and researches in electrical science. In the former character, our readers have become acquainted with his contributions to the knowledge of one of the most active and pervading, and at the same time most recondite of the principles which govern the material world, by the abstracts which have from time to time appeared in our pages, of his Experimental Researches in Electricity, as they have been communicated in successive series, to the Royal Society, and from our having reprinted several of those series entire; while as forming part of the scientific world at large, they must necessarily participate in the wide diffusion of his most laboriously earned reputation. We do not intend, on the present occasion, to offer any opinion of our own, on the value and character of Mr. Faraday's labours in electricity and the cognate sciences, but we will at once refer the matter to an arbiter, from whose award, in such a case, few will be disposed to appeal,—and stamp a character on the present article, by giving additional currency to the judgment pronounced by the Rev. Professor Whewell, who, in his "History of the Inductive Sciences," has affirmed, with respect to the great principle of the identity of electrical and chemical action, that "The confirmation of Davy's discoveries by Faraday is of the nature of Newton's confirmation of the views of Borelli and Hooke respecting gravity, or," [and this, we may remark, is scarcely inferior praise] "like Young's confirmation of the undulatory theory of Huyghens."

We have selected an opinion pronounced on the chemical bearings of Mr. Faraday's labours, because we think, that in consequence of the ease with which brilliant and even marvellous experimental results in the magnetic relations of electricity may now be obtained, his contributions to the science of *Electro-chemistry*, are perhaps, by many of the lovers of electricity as well as chemistry, at the present time not sufficiently attended to. It would have been easy, however, to have cited opinions not less emphatic, on Mr. Faraday's discoveries in magneto-electricity, the other great branch of the general science to which his results, in the first eight series at least, chiefly refer. Were we to offer an opinion of our own on the contents of the subsequent series, we should allude to the importance of the subject of Induced Electricity, as it has been treated by our author; in reference, especially, to the philosophy, in all cases, (whether electrical or not) of what has been called *distant action*. On this particular subject, one of almost universal extent, science, we are convinced, is yet pregnant with discovery.

The volume now before us consists of Mr. Faraday's Fourteen Series of Experimental Researches in Electricity, which have appeared in the Philosophical Transactions during the last seven years; his chief reason for their publication in this form, being stated, in the Preface, to have been "the desire to supply at a moderate price the whole of these papers, with an index, to those who may desire to have them." There are some other passages in the preface which

we think it desirable to cite, both in justice to the author, and on account of their intrinsic interest;—

"The readers of the volume will, I hope, do me the justice to remember that it was not written as a *whole*, but in parts; the earlier portions rarely having any known relation at the time to those which might follow. If I had rewritten the work, I perhaps might have considerably varied the form, but should not have altered much of the real matter: it would not, however, then have been considered a faithful reprint or statement of the course and results of the whole investigation, which only I desired to supply.

"I may be allowed to express my great satisfaction at finding that the different parts, written at intervals during seven years, harmonize so well as they do. There would have been nothing particular in this, if the parts had related only to matters well ascertained before any of them were written:—but as each professes to contain something of original discovery, or of correction of received views, it does surprise even my partiality, that they should have the degree of consistency and apparent general accuracy which they seem to me to present.

"I have made some alterations in the text, but they have been altogether of a typographical or grammatical character; and even where greatest, have been intended to explain the sense, not to alter it. I have often added Notes at the bottom of a page, for the correction of errors, and also the purpose of illustration: but these are all distinguished from the Original Notes of the Researches by the date of *Dec.* 1838.

"The date of a scientific paper containing any pretensions to discovery is frequently a matter of serious importance, and it is a great misfortune that there are many most valuable communications, essential to the history and progress of science, with respect to which this point cannot now be ascertained. This arises from the circumstance of the papers having no dates attached to them individually, and of the journals in which they appear having such as are inaccurate, i. e. dates of a period earlier than that of publication. I may refer to the note at the end of the First Series, as an illustration of the kind of confusion thus produced. These circumstances have induced me to affix a date at the top of every other page, and I have thought myself justified in using that placed by the Secretary of the Royal Society on each paper as it was received. An author has no right, perhaps, to claim an earlier one, unless it has received confirmation by some public act or officer."

In connexion with the general subject of the volume, Mr. Faraday then alludes to his papers on Electro-magnetic Rotations in the Quarterly Journal of Science for 1822, and on Magneto-electric Induction in the Annales de Chimie, vol. li., which, he remarks, "might, as to the matter, very properly have appeared in this volume, but they would have interfered with it as a simple reprint of the 'Experimental Researches' of the Philosophical Transactions." He next refers in relation to the Fourth Series of his own Researches, on a new law of electric conduction, to Franklin's experiments on the non-conduction of ice as brought forward by Professor Bache, observing, "These, ... though they in no way anticipate the expression of the law I state as to the general effect of liquefaction on electrolytes, still should never be forgotten when speaking of that law as applicable to the case of water."

"There are two papers which I am anxious to refer to, as corrections or criticisms of parts of the Experimental Researches. The first of these is

one by Jacobi, (Philosophical Magazine, 1838. xiii. 401.) relative to the possible production of a spark on completing the junction of the two metals of a single pair of plates (915.). It is an excellent paper, and though I have not repeated the experiments, the description of them convinces me that I must have been in error. The second is by that excellent philosopher, Marianini, (Memoria della Societa Italiana di Modena, xxi. 205) and is a critical and experimental examination of Series viii. and of the question whether metallic contact is or is not *productive* of a part of the electricity of the voltaic pile. I see no reason as yet to alter the opinion I have given; but the paper is so very valuable, comes to the question so directly, and the point itself is of such great importance, that I intend at the first opportunity renewing the inquiry, and, if I can, rendering the proofs either on the one side or the other undeniable to all.

"Other parts of these researches have received the honour of critical attention from various philosophers, to all of whom I am obliged, and some of whose corrections I have acknowledged in the foot notes. There are, no doubt, occasions on which I have not felt the force of the remarks, but time and the progress of science will best settle such cases; and, although I cannot honestly say that I *wish* to be found in error, yet I do fervently hope that the progress of science in the hands of its many zealous present cultivators will be such, as by giving us new and other developments, and laws more and more general in their applications, will even make me think that what is written and illustrated in these experimental researches, belongs to the by-gone parts of science."

An analytical table of contents succeeds the preface, which, on account of its general utility and as furnishing a clew to the distribution, through the Fourteen Series, of the several objects of specific research, we shall transfer to our pages.

"Series X. §. 16. Improved voltaic battery. §. 17. Practical results with the voltaic battery.

"Series XI. §. 18. On static induction. ¶ 1. Induction an action of contiguous particles. ¶ 2. Absolute charge of matter. ¶ 3. Electrometer and inductive apparatus. ¶ 4. Induction in curved lines. Conduction by glass, lac, sulphur, &c. ¶ 5. Specific inductive capacity. ¶ 6. General results as to the nature of induction. Differential inductometer.

"Series XII. ¶ 7. Conduction or conductive discharge. ¶ 8. Electrolytic discharge. ¶ 9. Disruptive discharge; insulation; as spark; as brush; positive and negative.

"Series XIII. Disruptive discharge as glow; dark. ¶ 10. Convection; or carrying discharge. ¶ 11. Relation of a vacuum to electrical phenomena. §. 19. Nature of the electric current; its transverse forces.

"Series XIV. §. 20. Nature of the electric force or forces. §. 21 Relation of the electric and magnetic forces. §. 22. Note on electrical excitation."

Next follow in order the "Experimental Researches in Electricity," from the First to the Fourteenth Series, illustrated by the original engravings, from the Philosophical Transactions.

The volume concludes with a copious and minutely particular index, occupying eighteen pages, and presenting, in fact, a complete analysis of the objects and results of the author's "Researches" contained in this work. This we are enabled to say from our own acquaintance with those "Researches," and examination of the index; which has also the recommendation of having been, as we happen to know, constructed by the author himself, a recommendation which every one having any practical experience in the bibliography and history of science and scientific discovery will know how to estimate.

As the Fifteenth Series of Mr. Faraday's Researches has already been read before the Royal Society, and will appear in the forthcoming part of the Philosophical Transactions, it is evident that his devotion to the subject is still unremitting, and we earnestly wish him health and happiness yet further to develope the laws of electrical action, hoping to have the pleasure of announcing to our readers, in due time, the publication of another and similar collection of his "Experimental Researches in Electricity," in sequence to the present volume.

LXX. *Intelligence and Miscellaneous Articles.*

SCIENTIFIC MEMOIRS, PART V.

PART V. of the Scientific Memoirs, being the first of the second volume, is just ready for publication. It commences with a paper by Jacobi on some Electro-Magnetic Experiments, forming a sequel to the Memoir on the Application of Electro-Magnetism to the Movement of Machines, of which a translation appeared in the first volume. The second article is one the importance of which will be readily appreciated at the present time; it is a translation, revised by Professor Lloyd and Major Sabine, of the "Results of the Observations made by the Magnetic Association in the year 1836," edited

by Professors Gauss and Weber, being the First Annual Report of the Magnetic Association. An introduction, from the pen of Gauss, on the Irregular Variations of the Terrestrial Magnetic Force, is succeeded by "Remarks on the Arrangement of Magnetical Observatories, and Description of the Instruments to be placed in them," by Weber; which is followed by a minute account of the "Method to be pursued during the terms of Observation," by Gauss. This is succeeded by an "Extract," also by Gauss, "from the daily Observations of Magnetic Declination during three years at Göttingen," and a "Description of a small portable apparatus for measuring the absolute intensity of Terrestrial Magnetism, and Explanations of the six graphical Representations and of the Table of Results." Our readers will remember the Report to the Council of the Royal Society of a Joint Committee of Physics and Meteorology "on the establishment of fixed Magnetic Observatories and the equipment of an Antarctic Expedition for Magnetic Observations," which appeared in the Philosophical Magazine for February last, and to which we may refer those who may not have already become acquainted with the value and bearings of the work of Gauss and Weber, which now first appears in the English language.

The Part, which is illustrated by Ten Engravings, also contains a highly valuable memoir by Professor Heinrich Rose on the Combinations of Ammonia with Carbonic Acid; and another by Melloni on the Polarization of Heat.

PREPARATION OF DICHLORIDE OF CARBON. BY M. REGNAULT.

M. Regnault prepared the proto-chloride of carbon, which, however, according to French equivalents, is described as CCl^2, according to Faraday's process; he states that he found its boiling point to be 248° instead of 170°, as mentioned by Faraday; the density of the vapour he ascertained to be 5·8, and therefore he considers it as composed of $C^4\,Cl^8$, and it belongs, he says, to the series of *chloride of aldehyde*,—that is, to a series of which it contains only two out of three elements,—but then this is explained by the doctrine of substitutions—"C'est l'hydrogène bicarboné $C^4\,H^8$, dans lequel l'hydrogène est remplacé par son équivalent de chlore." It appears to me that it would be quite as consistent with sound philosophy, and attended with the additional advantage of somewhat extending the *doctrine of substitutions*, if we were to say, that *water belongs to the series of sulphurets of mercury in which the sulphur is replaced by its equivalent of oxygen, and the mercury by its equivalent of hydrogen.*

M. Regnault appears, however, to have succeeded in preparing the dichloride of carbon, a specimen of which, as an accidental product, was examined by Mr. Faraday and myself. He procured it by repeatedly passing the proto-chloride through a tube heated to redness; the dichloride condenses in the coldest parts of the tube in very fine silky needles, which are to be separated by æther; when resublimed it is quite pure. This substance is nearly inodorous; it is difficult to hit upon the exact degree of heat for its preparation; if it be too great, the decomposition is complete and charcoal is deposited.—*R. P. An. de Ch. et de Ph.* lxx., 105.

DELVAUXENE—A NEW PHOSPHATE OF IRON.

This mineral was first found in 1793, at Berneau, near Visé; it occurs in brittle reniform masses, its texture is compact, and its fracture perfect conchoidal. It is opake or only slightly translucent on the edges of thin fragments; its lustre is sometimes resinous; sometimes it is dull; colour blackish or reddish brown, but sometimes yellowish brown; the powder is of a yellowish brown, and the finer the brighter. Its hardness is intermediate as to that of calcareous spar and sulphate of lime: specific gravity 1·85. When heated in a flask it yields much water, and loses 42 per cent. of water when heated to redness. Before the blow-pipe it decrepitates and fuses into a grey very magnetic globule of iron.

With borax on a platina wire, in the reducing flame, a bottle-green globule is obtained, and in the oxidating flame a globule, which is brownish while hot, and becomes green on cooling. In water it falls to pieces, effervesces and gelatinizes in hydrochloric acid, forming a brownish orange solution; the nitric solution gives a white precipitate with nitrate of lead, and a blue one with ferrocyanide of potassium. This mineral was first found in a lead mine, but it has since occurred in a stone-quarry near the same place. M. Dumont analyzed both varieties—1st, reddish brown, 2nd, brownish black—the results were

	No. 1.	No. 2.
Phosphoric acid	13·60	14·30
Peroxide of iron	29·00	31·60
Water	42·20	40·40
Carbonate of lime	11·00	9·20
Silica	3·60	4·40
	99·40	99·90

Neglecting the carbonate of lime and silica, this mineral is a diphosphate of peroxide of iron + 6 eqs. of water; the phosphate of iron of the Isle of France, analysed by Laugier, differs from the above in containing only half the quantity of water.

The name of Delvauxene was given to this mineral by M. Dumont from that of its discoverer M. Delvaux.—*L'Institut,* No. 276.

ON THE USE OF AMMONIA IN FIXING PHOTOGRAPHS. BY J. C. CONSTABLE, ESQ.

To the Editors of the Philosophical Magazine.

GENTLEMEN,

Mr. Fox Talbot, in his paper on photogenic drawing, states, that he did not succeed in preserving the drawings by means of ammonia; some experiments which I have made lead to a different result. I find that the drawings, after being soaked for some minutes in a moderately strong solution of ammonia and then washed in clean water, withstand the action of the light perfectly, and indeed are improved by it: for the first action of the ammonia is to make the dark parts of a reddish hue, which, on exposure to the light, become again of a dark colour, the light parts being unaffected. This mode

of preservation has, I conceive, advantages over those already used. Common salt never preserves completely so as to enable the drawings to withstand the action of the sun. Iodide of potassium seems to require great delicacy in management, as when at all too strong it eats out the fainter tints, and is moreover subject to this inconvenience—that sometimes the drawings so preserved, even when kept in the dark, become entirely bleached and lose all traces of the dark lines. This at least has happened to some drawings so prepared by a friend of mine. There is no doubt that the hyposulphite of soda is an excellent preservative, but it is a salt not easily prepared and not likely to be in the hands of those who may wish to make experiments on the subject. If you think these remarks worth publishing, I shall be obliged by their insertion in your next number.

I am, Gentlemen, your obedient servant,

J. C. Constable.

Jesus College, May 21, 1839.

APPLICATION TO PHOTOGRAPHY OF THE LIGHT OF INCANDESCENT COKE. BY MR. R. MALLET.

The following is an extract from the Proceedings of the Royal Irish Academy (No. 16), for April 22nd, 1839:

"Mr. Robert Mallet communicated a notice of the discovery of the property of the light emitted by incandescent coke to blacken photogenic paper; and propose it as a substitute for solar light, or that from the oxy-hydrogen blowpipe with lime.

"One of the most important applications of the photogenic process, as yet suggested, is its adaptation to the self-registering of long continued instrumental observations. Unless, however, an artificial light of a simple and inexpensive character can be found to supply the place of solar light at night, the utility of this application will be much limited.

"Few artificial lights emit enough of the chemical rays to act with certainty on the prepared paper; while those which are known to act well, as the oxyhydrogen lime light, are expensive and difficult to manage. A considerable time since the author discovered that the light emitted by incandescent coke at the 'Twyer' (or aperture by which the blast is admitted) of a cupola or furnace for melting cast iron, contained the chemical rays in abundance; and on lately trying the effect of this light on the prepared paper, he found it was intensely blackened in about forty-five seconds. In the single experiment made, the heat, which was considerable, was not separated from the light; but the author purposed to make further experiments, in which this precaution will be attended to.

"There is no difficulty to be apprehended in contriving an apparatus to burn a small quantity of coke at a high temperature. A diagram of an apparatus for this purpose was shown. It consists of a vertical tube, nine inches in diameter, lined with refractory clay, and closed at top and bottom. There is a grating about one foot from the bottom, a little above which are two opposite holes, into

one of which an air blast from a revolving fanner is projected through the coke, with which the whole tube is filled. The flame passes out at the opposite hole, through a tube so contrived, as to heat the blast of air to a temperature of 500°, just before it enters the coke fire.

"The light from the former lateral aperture is that proposed to be used, and issues through a plate of mica or glass opposite to it. This aperture forms part of the conductory tube for the blast, which (by passing into the coke in a direction opposite to that in which the light is emitted) keeps the illuminating surface of coke clear from ashes; these are received below the grating, and by a diversion of part of the blast, are blown into the chimney which receives the other products of the combustion.

"As the vertical tube is close above, the combustion cannot proceed upwards, while the coke with which it is filled constantly drops down to supply the place of that consumed, on the principle of the ancient furnaces, called 'athanors' by the earlier chemists.

"The only difficulty to be apprehended in the use of coke, is the collection of slag from the fusion of its earthy and ferruginous constituents; however the author does not consider that this accumulation during the period from sunset to sunrise, in mid-winter, would materially interfere with its action."

ON THE COMPOSITION OF IDOCRASE. BY H. HESS.

Mineralogists have not yet hitherto agreed as to the composition of idocrase; only so far is certain, that many consider the chemical formulæ to be the same as that of garnet. Under this idea garnet and idocrase are considered to be the same substance under two different forms. However this is not the case. Possessing a very fine crystal of idocrase from Slatoust, I gave it to one of my best pupils, M. Ivanor, to analyse. The following is the result of his analysis:

				Oxygen.	
Silica..........	37·079	containing	....		19·262
Alumina	14·159	,,	....	6·612	
Lime	30·884	,,	8·644		19·621
Protoxide of iron	16·017	,,	3·646	13·009	
Magnesia	1·858	,,	0·719		
	99·997				

which gives exactly $2(\dot{Ca}^3\ \dot{Fe}^3\ \dot{Mg}^3)\ \dddot{Si} + \underline{\dddot{Al}}\ \dddot{Si}$.

We therefore possess three mineral species which only vary in the number of their compounded elements, viz.

Garnet .... $\dot{R}^3\ \dddot{Si} + \underline{\dddot{Al}}\ \dddot{Si}$

Idocrase.......... $2\,\dot{R}^3\ \dddot{Si} + \underline{\dddot{Al}}\ \dddot{Si}$

Epidote .... $\dot{R}^3\ \dddot{Si} + 2\underline{\dddot{Al}}\ \dddot{Si}$.

There can be no doubt as to the correctness of M. Ivanor's analysis, as it was performed under my superintendence and upon a

quantity unknown to the analyst. As the different weights agreed with the original weight of the mineral I had given out for analysis, no error could have crept into the result.—*Poggendorff's Annalen*, No. 10, 1838.

ON THE COMPOSITION OF IDOCRASE FROM SLATOUST. BY F. VARRENTRAPP.

Klaproth, von Robell, and particularly Magnus, have shown, after very careful analysis, that idocrase and garnet have the same chemical composition. Notwithstanding that every attention has been directed to find an essential chemical difference to account for the variation in the crystalline form, it has been hitherto in vain. Ivanor's analysis, as reported in the Transactions of the St. Petersburg Academy, of an idocrase from Slatoust in Siberia, has attracted attention, as the result is not in accordance with that of other chemists, and the difference is such as to require a different formula. The locality was the same as the idocrase examined by Magnus. G. Rose having brought a quantity of this mineral from Siberia, Mr. F. Varrentrapp repeated the analysis, with the following results:

	Varrentrapp.	Magnus.	Ivanor.
Silica	37·55	37·178	37·079
Alumina	17·88	18·107	14·159
Lime	35·56	35·790	30·884
Protoxide of iron	6·34	4·671	16·017
Magnesia	2·62	2·268	1·858
	99·95	98·024	99·997

The mineral consisted of well-formed transparent green crystals imbedded in felspar, from which they were easily separated, exactly the same as those described by Magnus in his analysis. The crystals were carefully selected, reduced to fine powder, heated with carbonate of soda, and examined in the usual manner. The great difference in the results between M. Varrentrapp and M. Magnus on the one side, and M. Ivanor on the other, is difficult to account for. It may have arisen in M. Ivanor's analysis in the use of too small a quantity of potash in the separation of the alumina from the oxide of iron, which would give a larger proportion of oxide of iron and a smaller one of alumina; the deficiency in the quantity of lime may have arisen from not having quickly enough filtered the solution after precipitating by ammonia.

Fuchs observed as long ago as in 1818, that idocrase, as well as several other minerals containing an alkali or an alkaline earth, when subjected to a strong heat and fused, were capable of being decomposed by hydrochloric acid, forming a gelatinous mass. Von Robell and Magnus confirmed this observation, and the latter further observed that the specific gravity of idocrase after fusion was considerably diminished, although no alteration in the composition could be discovered.

M. Varrentrapp in making a second analysis of this mineral first fused it; he obtained a good flowing clear glass of a brown colour, of

which a portion was reduced to powder and acted upon by hydrochloric acid, which soon brought it into a gelatinous state. The result of this analysis was

Silica	37·84
Alumina	17·99
Lime	35·18
Protoxide of iron	6·45
Magnesia	2·81
	100·27

The mean specific gravity from four weighings of the crystallized idocrase was 3·346, that of the fused, agreeing with Magnus, was 2·929—2·941.—*Poggendorff's Annalen der Physik und Chimie,* No. 10, 1838.

TERRESTRIAL MAGNETISM.

In a memoir presented by M. Quetelet to the Royal Academy of Brussels, in which he examines the results of the observations which he has made during twelve years upon the state of terrestrial magnetism at Brussels, the result of the whole of these observations is stated to be, that the magnetic needle has been constantly approaching the meridian line, that is to say, that the declination and the dip have diminished from year to year, contrary to what had been observed before, at least as to magnetic declination. The results observed were as follows:

Date.	Declination.	Dip.
1827 October	22° 28′ 8	68° 56, 5
1830 end of March	22 25, 3	68 52, 6
1832 ,,	22 19, 0	68 49, 1
1833 ,,	22 13, 4	68 42, 8
1834 beginning of April	22 15, 2	68 38, 4
1835 end of March	22 6, 7	68 35, 0
1836 ,,	22 7, 6	68 32, 2
1837 ,,	22 4, 3	68 28, 8
1838 ,,	22 3, 7	68 26, 1
1839 ,,	21 53, 6	68 22, 4

This year's observations for the declination were made on the 29th of March. The value indicated is the mean of two series of observations, which gave successively 21°53′,1 and 21°54′,2. Similar observations had been made the evening before, in less favourable circumstances, on account of the agitation of the air. The latter had given for the declination 21°51′,3 and 21°51′,1: it was thought that preference should be given to the former. In these different series of observations the meridian was determined by placing the magnetic apparatus previously in such a manner that the telescope might be directed at pleasure on the middle wire of the transit of the observatory.

The value of the dip is the mean of the three following amounts, 68°22′,25, 68°22′,67, and 68°22′,25, obtained in succession on the 31st of March. These observations were made under very favourable circumstances. As in preceding years, it was thought proper tc make the different observations for the declination and the dip at the same time of year and about the same hours of the day.

NOTE ON THE UNDULATORY THEORY OF LIGHT. BY JOHN TOVEY, ESQ.

To the Editors of the Philosophical Magazine.

Gentlemen,

I perceive that the formulæ (18) of my paper in your present vol., p. 171, appear to lead to contradictory results; I shall be glad, therefore, if you will allow me to say, that this matter will be cleared up in my next paper, which will contain an improved method of finding the general integrals.

I am, Gentlemen, yours, &c.,
John Tovey.

Littlemoor, Clitheroe, May 13th, 1839.

METEOROLOGICAL OBSERVATIONS FOR APRIL, 1839.

Chiswick.—April 1. Rain. 2. Overcast. 3, 4. Bleak and cold. 5. Snowing. 6. Cloudy and cold. 7. Fine. 8. Snowing. 9. Bleak and cold. 10, 11. Fine but cold. 12—14. Cloudy and cold. 15. Overcast. 16. Very fine. 17. Showery. 18. Boisterous with rain. 19. Very fine. 20. Showery. 21. Fine. 22. Very fine. 23. Rain. 24—26. Fine. 27. Dry haze. 28—30. Very fine.

Boston.—April 1. Fine. 2. Stormy. 3—7. Cloudy. 8. Cloudy: sleet early A.M. 9. Cloudy. 10. Fine. 11—15. Cloudy. 16. Fine. 17, 18. Rain. 19. Fine: rain early A.M. 20, 21. Fine: rain A.M. and P.M. 22. Fine: rain early A.M. 23. Rain. 24—26. Cloudy. 27. Cloudy: rain A.M. 28—30. Fine.

Applegarth Manse, Dumfries-shire.—April 1. A most inclement day: snow on hills. 2. The same: snow on hills melting. 3. The same: bitterly cold. 4. Another piercing day: cloudy P.M. 5. Still extremely cold: snow showers. 6. Wind fallen: more temperate. 7. Moderate day: still no vegetation. 8. Piercingly cold and withering. 9. Dry and cold: frosty mornings. 10. Sun warm, but wind cold and withering. 11. Milder, but still no spring. 12. Great increase of temperature. 13. Sun warm: wind moderate but parching. 14. Moderate day: vegetation commencing. 15. The same: temperature lower: cloudy. 16. Threatening rain: showery: very wet P.M. 17. Showers: rain: hail: cleared P.M. 18. Frequent showers: rain and sleet: snow. 19. Violent wind: showers of hail. 20. Dry and cold: vegetation at a stand. 21. Dry: temperature rising. 22. Foggy morning: drizzling day. 23. Clear: temperature increasing. 24. The same: cool evening. 25. Temperature increasing: clear sun. 26. Cloudy: threatening. cleared up P.M. 27. Clear and fine: hoar frost morning. 28. The same: cloudy P.M. 29. Fine spring day. 30. Remarkably fine spring day.

Sun 25 days. Rain 4 days. Snow 2 days. Hail 2 days. Frost 3 mornings.
Wind easterly 13 days. Southerly 12 days. Northerly 2 days. Westerly 3 days.
Calm 11 days. Moderate 7 days. Strong breeze 4 days. Stormy 5 days. Brisk 3 days.
Mean daily range of barometer 0·092. Mean nightly range 0·080. Mean range of 24 hours 0·172.
Mean daily range of thermometer 10·4.

Meteorological Observations made at the Apartments of the Royal Society by the Assistant Secretary, Mr. Roberton; *by* Mr. Thompson *at the Garden of the Horticultural Society at Chiswick, near London; by* Mr. Veall *at Boston, and by* Mr. Dunbar *at Applegarth Manse, Dumfries-shire.*

Days of Month. 1839. April.	Barometer. London: Roy. Soc. 9 a.m.	Barometer. Chiswick. Max.	Barometer. Chiswick. Min.	Barometer. Boston. 8½ a.m.	Barometer. Dumfries-shire. 9 a.m.	Barometer. Dumfries-shire. 8½ p.m.	Thermometer. London: Roy. Soc. Fahr. 9 a.m.	Thermometer. London: Roy. Soc. Self-register. Max.	Thermometer. London: Roy. Soc. Self-register. Min.	Thermometer. Chiswick. Max.	Thermometer. Chiswick. Min.	Thermometer. Boston. 8½ a.m.	Thermometer. Dumfries-shire. 9 a.m.	Thermometer. Dumfries-shire. 9 pm.	Wind. London: Roy. Soc. 9 a.m.	Wind. Chiswick 1 p.m.	Wind. Bost.	Wind. Dumfries-shire.	Rain. London: Roy. Soc. 9 a.m.	Rain. Chiswick.	Rain. Boston.	Rain. Dumfries-shire.	Dew point. Lond.: Roy. Soc. 9 a.m.
1.	29·586	29·672	29·601	29·27	29·66	29·83	41·7	46·3	40·7	45	38	43	40	34	E.	E.	E.	E.	·116	·07	...	...	38
2.	29·766	29·929	29·761	29·52	29·98	30·08	38·8	41·0	38·9	39	37	40	36	33	NE.	E.	E.	E.	·088	...	...	...	36
3.	29·896	29·974	29·910	29·64	29·99	30·03	35·9	36·6	35·6	36	31	35	35	32	NE.	NE.	E.	E.	...	...	...	...	33
4.	29·986	30·028	29·939	29·67	29·98	29·94	35·5	36·0	33·9	37	32	36	35½	31½	NNE.	NE.	E.	E.	...	...	...	...	30
5.	29·802	29·995	29·827	29·54	29·83	29·99	33·5	34·3	34·0	41	34	37·5	36½	32	NE.	E.	E.	E.	·061	·56	...	0·00	31
6.	30·158	30·358	30·154	29·88	30·16	30·32	40·2	40·7	34·0	43	24	38	38½	32	NNE.	NE.	E.	SE.	·340	...	...	...	33
☾ 7.	30·398	30·417	30·413	30·04	30·40	30·37	38·6	39·3	30·5	49	31	38	36½	29½	NNE.	E	E.	ESE.	...	...	...	...	29
8.	30·320	30·348	30·333	29·88	30·38	30·42	40·3	41·4	34·8	43	32	40	38½	27½	NNE.	E.	E.	ESE.	...	·01	...	...	32
9.	30·352	30·434	30·373	30·05	30·50	30·60	38·8	39·7	35·0	41	36	40	36½	30½	NNE.	E.	E.	E.	...	...	...	...	32
10.	30·444	30·550	30·455	30·10	30·65	30·65	42·4	43·2	37·3	52	29	43	39½	27½	NE.	E.	E.	ENE.	·027	...	...	...	33
11.	30·518	30·543	30·442	30·11	30·67	30·54	41·4	42·7	33·2	53	37	40·5	36½	28	NNE.	NE.	E.	ENE.	...	...	...	...	33
12.	30·282	30·307	30·228	29·82	30·43	30·34	42·6	43·3	37·2	50	42	44	50	35½	N.	NE.	NE.	E.	...	...	...	...	37
● 13.	30·272	30·278	30·242	29·80	30·29	30·21	44·8	45·3	41·3	47	41	46	50	42½	N.	N.	NE.	E.	...	...	...	...	38
14.	30·232	30·247	30·215	29·75	30·23	30·20	45·7	46·4	43·7	55	42	45	48	37	W.	NW.	calm	SSW.	...	...	...	0·50	39
15.	30·138	30·140	29·975	29·58	30·05	29·84	48·5	49·3	45·2	53	40	50	44	42½	SW.	SW.	calm	S.	...	...	...	...	39
16.	29·792	29·836	29·535	29·27	29·63	29·44	50·2	50·8	44·6	60	44	52	43	41	S.	S.	calm	S.	...	·01	...	...	37
17.	29·436	29·457	29·409	28·83	29·24	29·27	48·4	49·4	45·5	55	39	46	41½	40½	W. var.	SW.	SW.	SSW.	...	·32	...	...	42
18.	29·450	29·578	29·491	28·97	29·27	29·36	47·2	47·6	40·9	58	46	43·5	42½	35	S.	SW.	SW.	W.	·205	·20	·14	...	42
19.	29·662	29·965	29·646	29·05	29·50	29·74	51·4	52·0	40·7	59	33	48	44	33	W.	W.	W.	W.	·180	...	·21	...	45
☽ 20.	30·082	30·235	30·069	29·52	29·95	30·13	49·8	50·7	40·8	55	37	47·5	47	37	W.	NW.	W.	NW.	...	·01	...	...	42
21.	30·260	30·273	30·260	29·67	30·18	30·22	48·7	49·5	41·3	55	32	48	47	39½	WNW.	W.	calm	W.	·033	...	·03	...	40
22.	30·268	30·270	30·168	29·76	30·20	30·07	52·3	53·5	42·7	61	46	50	43	39½	S.	W.	calm	SW.	...	·16	...	...	43
23.	29·984	30·004	29·958	29·40	29·92	30·03	51·3	51·8	47·4	55	39	52	49½	45	S.	W.	calm	ENE.	·155	·12	·30	...	46
24.	30·078	30·111	30·075	29·57	30·06	30·12	45·8	46·4	42·3	55	28	45	48	44	NW.	N.	N.	ENE.	·172	...	...	...	41
25.	30·074	30·090	30·038	29·58	30·07	30·04	44·6	46·0	36·9	63	34	45	49	40	NW.	NE.	calm	SE.	...	...	...	...	38
26.	30·148	30·219	30·139	29·65	30·08	30·15	44·3	50·4	39·2	54	38	43·5	47	45½	NNE.	SE.	calm	S.	...	...	...	0·48	37
27.	30·238	30·262	30·234	29·70	30·20	30·24	48·7	49·3	43·6	61	38	52	47	33½	SE.	SW.	calm	SSW.	...	...	...	...	43
○ 28.	30·340	30·335	30·313	29·77	30·30	30·28	47·8	49·4	42·0	63	30	54	51	37	NNW.	E.	calm	SSW.	...	...	·07	...	42
29.	30·250	30·262	30·182	29·67	30·10	30·10	48·8	49·9	42·6	68	37	56·5	50½	46	E.	E.	calm	SW.	...	...	...	...	43
30.	30·148	30·145	30·039	29·62	30·05	30·02	57·3	58·7	46·7	73	41	56·5	56	49½	E.	NW.	calm	S.	...	...	...	...	45
Mean.	30·078	30·142	30·047	29·62	30·108	30·088	44·8	46·0	39·7	52·63	36·26	45·1	43·5	36·7					Sum. 1·377	1·46	0·75	0·98	Mean. 37·9

THE
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SUPPLEMENT TO VOL. XIV. THIRD SERIES.

LXXI. *On the Expansive Action of Steam in some of the Pumping Engines on the Cornish Mines.* By WILLIAM JORY HENWOOD, *F.G.S., Secretary of the Royal Geological Society of Cornwall, H. M. Assay-Master of Tin in the Duchy of Cornwall**.

THE experiments which it is my purpose to describe, were instituted with a view to the determination of the quantity of steam employed, and the mode of its distribution on the working stroke; the duty performed with a given quantity of fuel; and the work accomplished for a certain expense.

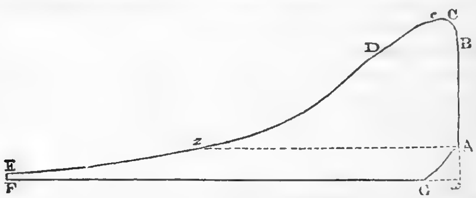
I. *The quantity of steam employed, and the mode of its distribution on the working stroke*, were approximated to by the use of an indicator, lent me for the purpose by Robert Were Fox, Esq. It consists of a brass cylinder about 11 inches long, and 1·6 inch in diameter, open at both ends, and accurately fitted with a piston, which, when at rest, is retained near the middle of the cylinder by a spiral spring, of which one end is attached to the piston, and the other to the top of the cylinder: the upper extremity of the piston-rod is provided with a receptacle for a pencil. A tapered stop-cock is fixed on the lower end of the cylinder, and is introduced into the grease-hole or other aperture in the cylinder-cover of any engine on which the indicator is placed. A light frame of wood, about 18 inches long and 4 inches wide, is fastened to the top of the indicator-cylinder, and in it a small board slides horizontally in grooves.

During the working stroke of the engine a direct motion is given to the slider by means of a string which passes over a pulley, and is connected with the radius-rod of the parallel motion. Its return is effected by the action of a counterpoise suspended over a similar small wheel. On this moveable board a piece of paper is firmly secured, and a pencil is placed on the top of the piston-rod of the indicator.

* The Telford Medal of the Institution of Civil Engineers was awarded to this communication, which appears in the second volume of their Transactions.

Let us now examine the operation of a single-acting engine, and the movements of an indicator fixed on it.

Every thing being at rest, the piston of the engine at the top of the cylinder, and the point of the pencil standing at A, (in the figure below) steam is admitted from the boiler above the piston of the engine; the piston of the indicator is forced upwards, and the line A B is described by the pencil. The engine now begins to move, but so slowly that the steam enters from the boiler more rapidly than the piston recedes before it; its pressure in the cylinder, therefore, still increases, and the piston of the indicator continues to rise: but as the working stroke of the engine commences, the slider moves in the direction GF, and the compound of the two motions generates the line BC. At C the space left by the descent of the piston is exactly filled by the steam, which enters from the boiler in the same time; the indicator-piston, therefore, does not stir; but as the engine moves, the slider still advances in the same direction, (GF,) and the horizontal line C*c* is produced. The piston now acquires speed, whilst the steam (in the boiler having expanded) enters the cylinder with diminished velocity, and is insufficient to fill the enlarging space and still re-



tain the same density: it therefore expands, and the piston of the indicator descends, whilst the slider still moves in the same direction, and the curve *c*D is delineated. At D the steam valve, through which the steam from the boiler enters the cylinder, is closed, but the piston of the engine still descends by virtue of the elasticity of the steam already introduced, and of the momentum acquired by the moving parts of the machine. Whilst the steam expands, the indicator-piston descends, and as the same horizontal motion of the slider still continues, the parabolic curve DE is made by the pencil.

The equilibrium valve, which connects the upper part of the cylinder with the lower, is now opened; and as the steam thus presses equally on both sides of the piston, the working stroke terminates, and the return stroke is made: the motion of the slider is at the same time reversed.

But when this valve is opened, the pipe which connects the top of the cylinder with the bottom, and consequently a larger space, is open to the steam, and as the slider remains for the instant stationary, the indicator-piston descends through the small vertical line EF.

The return stroke is effected by the weight of the pump-rods alone; the pressure of the steam contained in the cylinder, therefore, remains unaltered, the indicator-piston is unmoved, and the line FG, described by the pencil, is perfectly horizontal.

But shortly before the termination of the return stroke, the equilibrium valve is closed, and the steam in the cylinder not being of sufficient elasticity to sustain the load of the engine, that portion of it which is contained between the upper surface of the piston and the cylinder-cover is compressed between them by the ascent of the former, until it is of force enough to support that weight; the return stroke is thus terminated, and the engine stops an instant or two before it commences another working stroke. This compression of the steam contained in the upper part of the cylinder forces the indicator-piston upward, and the resultant of this gradual elevation, and of the continued retrograde motion of the slider, is the small curved line GA, the pencil at the end of the stroke returning to and standing at A.

It is evident that the form of the portion ABC*c*D, which is produced during the admission of steam from the boiler on the piston, must depend on the load of the engine, its size, the dimensions of the steam valve, the pressure of steam in the boiler, and the capacity of the boiler itself, and that it will, therefore, vary as these particulars may differ.

The part D*z*E will deviate from a true parabola only when the steam in the cylinder is heated by being surrounded by a steam-case, or jacket, or by flues containing warm air, or cooled by the influence of the circumambient medium; consequently it will be generally pretty much alike in all cases. The same reasons and influences are equally applicable to the small and nearly vertical line EF, and to the longer horizontal one FG.

But, theoretically speaking, the curve GA is of more importance than any other portion of the figure; because it clearly shows what proportion of the working stroke is performed by the beneficial influence of working expansively.

For were the steam from the boiler admitted on the piston during the whole of the working stroke, or the pressure of the steam (if worked expansively) sufficient to support the load at the termination thereof, then the line FG, described

by the return stroke, would be prolonged horizontally until it intersected an extension of the vertical line AB at x; at which point the pencil would rest at the end of the return stroke, and the instant the equilibrium valve closed the engine would stop. But it has been seen that the engine continues to move, and that the indicator-piston rises and generates the curve GA, after that valve is closed: which circumstances clearly demonstrate that the steam included between the cylinder-cover and the upper surface of the piston, is meanwhile undergoing compression; and that its elasticity both at the conclusion of the working stroke, and at the closing of the equilibrium valve, was insufficient to sustain the load. And it follows, that the portion of the working stroke which has been performed after the steam has expanded so much as to be unequal to supporting the burden, must have been accomplished by the momentum acquired in the early part of the stroke. When the pencil rests at A, the force of the steam balances the load of the engine; for the piston is never permitted to rise so far as to touch the cylinder-cover. If, therefore, from A a line be drawn parallel to FG, until it cuts the parabolic curve DE, the point of intersection, z, will be at that part of the stroke where the (simple) elasticity of the steam and the load of the engine are exactly in equilibrio; and the portion zE, (described after the steam has so far expanded as to be insufficient to support the burden,) will denote the amount of benefit obtained by working expansively.

The only case in which I have been able to submit the results thus obtained with the indicator to a direct comparison with the quantity of water evaporated in the boilers was at Huel Towan, where 847·5 cubic feet of water were converted into steam. This would give 342,858 feet of steam of a pressure of 64·1 lbs. on the square inch, (or 49·1 lbs. on the inch above the atmosphere,) the mean pressure in the boiler during the experiment, or 2,153,647 cubic feet of the pressure of 10·2 lbs. on the inch*. The capacity of the cylinder-nozles and other parts of the engine which required to be filled with steam from the boiler at every stroke, was 355·57 cubic feet†, and the number of strokes made during the observations 7881. Therefore, if it were indispensable for the steam on the piston, at the termination of the working stroke, to be of elasticity sufficient to sustain the load of the engine,

* 10·2 lbs. was the load of the engine per square inch of the area of the piston.

† Brewster's Edinburgh Journal of Science, O. S. IX. p. 160; from this, however, the dimensions of the piston-rod, probably about 2 cubic feet, should be deducted.

2,802,247 cubic feet (of a pressure of 10·2 lbs. on the inch) would have been requisite; whereas but 2,153,647 cubic feet only could be obtained from the quantity of water evaporated. Consequently but the 0·768th of the contents of the cylinder, &c., could, on an average, have been filled with steam of that force; and the remaining 0·232 of the stroke must therefore have been performed by virtue of the momentum acquired by the machine in the early part of the working stroke.

This 0·232 part of the whole is therefore the benefit obtained by working the steam expansively; although the result obtained by the indicator exhibits a still greater (about 0·388) advantage. The cause of this difference it is not very easy to assign satisfactorily. It is just possible that it may be from the fluctuating pressure of the steam (from 77·25 to 47·22 lbs. on the inch) during the experiment, giving a result differing on a mean more than 61·8 lbs. on the inch, (the force when the curve represented in the figure p. 482 was obtained,) does from the average elasticity during the observation (64·1 lbs.). But perhaps it may more probably be from the steam, even when expanded to a less force than 10·2 lbs. on the inch, still exercising a beneficial influence in assistance of the momentum by which the latter part of the working stroke is performed.

In a first attempt at such a comparison, which I believe is here made, it may perhaps excite no great surprise that there is not a more exact coincidence between the results obtained by these very different modes of inquiry.

II. *The duty performed with a given quantity of fuel.*—The experiments with an object to determining the duty performed with a known quantity of fuel, were made on Wilson's engine at Huel Towan; on Swan's engine at Binner Downs Mine; and on Hudson's engine at East Crinnis Mine*. These were among the best engines in Cornwall, and they were selected on account of the very varied circumstances under which they worked.

At Huel Towan the cylinder with its cover and bottom were surrounded with a case or jacket, filled with dense steam from the boiler; and these, with the steam-pipes, nozles, &c., were covered with saw-dust from 16 to 20 inches deep. The boilers had a layer of ashes, of about the same thickness, placed on them.

There was no steam-case at Binner Downs, but there were small fires on each side of the cylinder, and the flues from them were carried spirally round it; another little fire was placed beneath the steam-nozle, from the boiler, and its flue

* The engineers were respectively, Mr. Grose, Messrs. Gregor and Thomas, and Mr. Sims.

was passed over the cylinder-cover; under the steam-pipe from the boiler was a similar fire, and its smoke was conveyed round the pipe for some distance. Such parts of the engine as were not enveloped by the flues were surrounded with saw-dust*, and the boilers were covered with ashes as at Huel Towan.

The engine at East Crinnis had neither steam nor heated air passed round it; but every part which contained dense steam was surrounded with a very thick covering of saw-dust, and the boilers were protected in a similar manner to those of the other engines.

On all these the indicator was placed; and also on Burn's engine at Binner Downs, which is inclosed in a similar manner to Swan's engine on the same mine, already mentioned; and on Trelawny's and Borlase's engines at Huel Vor, both which have steam-cases and other coverings like that described at Huel Towan. On the duty of these no experiments were made.

Table I.—(Constants.)

Dimensions of the Engines, and amount of their loads.

Mines and Engines.	Diameter of Cylinder.	Stroke in		Air-pump.		Diameter of valves.			Total load of the Engine.	Load per square inch of area of piston.
		Cylinder.	Pump.	Diameter.	Stroke.	Steam.	Equilibrium.	Exhausting.		
	In.	Feet.	Feet.	Inches.	Feet.	In.	In.	In.	lbs.	lbs.
Huel Towan, Wilson's	80	10	8	36	4	8	12	16	68666·4	10·2
Binner Downs, Swan's	70	10	7·5	33	4	9	12	16	724·3† 51967·7	10·23
———— Burn's	64	9·33	7·75	25	4·66	7	12	13	41345	10·7
East Crinnis, Hudson's	76	10·25	7·16	two, each 26	4·5	10	14	16	74086·1	11·4
Huel Vor, Trelawny's	80	10	7·5	two, each 24	3·5	9	14	16	98770	14·7‡
———— Borlase's	80	10	8	two, each 24	4	10	14	16	76010	12·1‡

* In the progress of my experiment, the saw-dust on the cylinder-cover ignited several times. The influence exercised on the steam within the cylinders by the media with which they were surrounded, may be discovered by an inspection of the diagrams. (Trans. Inst. C. E. Vol. ii. Figs. 4., &c., Pl. IV.)

† The stroke in this pump is but 5·5 feet.

‡ From Captain Lean's "Monthly Reports."

Table II.—(Variables.)

Quantities of water and steam, pressures of steam, and temperatures.

Mines and Engines.	Water in Boilers*. Cubic feet.			Steam in Boilers*. Cubic feet.			Pressure of Steam in the Boilers, lbs. per square inch.			Temperature of Hot-well†.		
	Greatest.	Least.	Mean.	Greatest.	Least.	Mean.	Greatest.	Least.	Mean.	Greatest.	Least.	Mean.
Huel Towan, Wilson's	1096	984	1080	796	684	700	77·25	47·25	64·1	100·5°	90°	93·8°
Binner Downs, Swan's	686	586	636	400	300	350	74·78	58·07	67·87	98	84	89·24
———, Burn's			884			230			55			
East Crinnis, Hudson's	2000	1820	1920	730	550	650	36·82	26·32	31·68	90	86·5	88·2
Huel Vor, Trelawny's			1164			792			47			82
———, Borlase's			2290			734			40			

Mines and Engines.	Temperature of Condensing Water.			Temperature of Boiler-shed.			Temperature of Engine-room.			Temperature of external Air.		
Huel Towan, Wilson's	66·5°	62°	64·72°	78·5°	74·75°	76°	77·5°	70°	75·28°	56·5°	52·5°	53·84°
Binner Downs, Swan's	56	50	52·32	108	73·75	76	73	64	66·48	56·5	49	52·56
———, Burn's												
East Crinnis, Hudson's	67·5	66	67	68	64·5	66·2	64	55	61·8	50	40·75	45
Huel Vor, Trelawny's												
———, Borlase's												

Note to Table II.

The following are the dimensions of the heating surfaces of the boilers of the three engines which were the principal subjects of my experiments. I add those of Loam's engine, on the United Mines, (with which I have been favoured by William Francis, Esq., the scientific director of that extensive mining establishment,) as the only machine the evaporation in which has been published. See Mr. Lean's Report in the *Cornwall Polytechnic Society's Transactions,* IV. (1836) p. 34.

Mines and Engines.	Area of the Fire-grates.	Surface exposed to action of the flame.	Total heating Surface exposed.
	Feet.	Feet.	Feet.
Huel Towan, Wilson's Engine.	72	114	2600
Binner Downs, Swan's	48	76	1440
East Crinnis, Hudson's	37·5	57	2500
United Mines, Loam's	49·5	98	2310

Loam's engine, at the United Mines, has the steam cylinder of 85 inches in diameter, the stroke in it is 10 feet, and in the pump 7·5 feet; the load is about 12 lbs. per square inch of the area of the piston, and the velocity about 4·8 strokes per minute: the elasticity of the steam employed I am

* The boilers were, of course, always full of water and steam; and as the quantity of one increased, that of the other diminished, and *vice versâ.*

† As the pressure of the steam in the boilers increased, the temperature of the hot-well declined; so that by observing the alteration in one, that of the other could be predicted with great certainty.

unable to state. From the 2nd of March to the 5th of August, 1836, the duty was about 65 millions of pounds lifted one foot, by 100 lbs. of coal, and the evaporation by the same quantity of fuel for the same period was 15·4 cubic feet. This is a sufficient approximation to the result which I had five years previously obtained at Huel Towan.

The stroke in the cylinder of Loam's engine is estimated at 10 feet; an apparatus is fixed on it for registering the actual space passed over, and the mean for five months was 9·913 feet.

Table III.—(Constants.)

Dimensions of the Pumps.

	Huel Towan, Wilson's Engine.			Binner Downs, Swan's Engine.			East Crinnis, Hudson's Engine.		
	Length of Pump. Feet.	Diameter of Pump. Inches.	Temperature of water in Pump *.	Length of Pump. Feet.	Diameter of Pump. Inches.	Temperature of water in Pump*.	Length of Pump. Feet.	Diameter of Pump. Inches.	Temperature of water in Pump*.
First lift, or set of pumps, from the surface	265·75	13	71°·125	21·25†	10·	89°·24‡	39·16	13	63°
Second......................	263·75	15·875	71·75	242·66	18·875	72·5	159·25	18	63
Third	197·75	16·125	71·875				269·66	18	63
Fourth......................	113·66	16·125	72·25				198·583	17	62·5
The deepest, which reaches to the bottom of the shaft.......	58·16	12·5	74	248	17·125	74	73·25	14	63

The whole loads of the three engines of which it was intended to ascertain the duty were raised perpendicularly, except the deepest lift of Wilson's engine at Huel Towan; and this was inclined to the horizon about 70°, and was connected to the engine-rod by a chain passing over two small wheels respectively of 9 and 16 inches in diameter.

The lowest lifts at Huel Towan and East Crinnis were lifting pumps, and their loads were raised by the working strokes of their respective engines. All the other pumps were forcing pumps (plungers), and their columns were lifted during the return strokes of the engines, by the weight of the rods§.

At Huel Towan, from the surface to a depth of about 534 feet, the connecting rods were 14 inches square; and from

* No correction has been applied for temperature, nor for impurities contained in the water. At Huel Towan I found, by evaporation, that about 360 grains were contained in a cubic foot. The temperature is higher as we descend; which adds to the already abundant evidence of the great heat prevailing in the interior of the earth.

† The stroke in this pump is but 5·5 feet.

‡ This *lift* took its supply from the hot-well.

§ The rods are usually very much heavier than the column of water, and a counterpoise is applied to balance some part of their weight: such was the case in all the engines here mentioned.

that place downward they extended about 300 feet, and were 12 inches square. They were kept in their places by thirteen sets of guides, which exposed a surface of about 53·5 square feet*.

From the surface to 396 feet deep in Binner Downs, the rods were 14 inches square; and from thence downward, there were about 258 feet of 12 inch rods; these were also retained by thirteen sets of stays, having an area of about 35·6 feet.

The rods, from the surface to 470 feet deep in East Crinnis, were 15 inches square, and thence about 200 feet deeper they were 12 inches: eleven sets of stays retained them in their places, and exposed a surface of about 38·8 feet.

Where the rods touch the stays they are protected by thin planks of some hard wood, which are always well covered with grease; they seldom fit very accurately.

Table IV.

Duration of the experiments, number of strokes made, materials consumed, &c.

Mines and Engines.	Duration of Experiments.	Coal consumed. Number of measured bush.	Coal consumed. Total weight.	Proportion of moisture in total weight:	Quantity of oil used (in the Engine.)	Quantity of grease used. In Engine.	Quantity of grease used. In Shaft.	No. of strokes made by the Engine.	Strokes per minute.	Duration of the working stroke.	Duration of the return stroke.	Interval between strokes.	Total quantity of water evaporated.	Water evaporated by 100 lbs. of coal.
	1831. h. m.		lbs.		pint.	lbs.	lbs.			s.	s.	s.		Cubic Feet.
Huel Towan, Wilson's.	22 Nov. 2 32 P.M. to 23 Nov. 3 5 P.M.	50	5003	$\frac{1}{16}$	1	17	3	7881	5·35	1·6	4·8	4·8	847·5	16·95
Binner Downs, Swan's	8 Dec. 10 59 A.M. to 9 Dec. 0 2 P.M.	60	5561	$\frac{1}{10}$	1	12·5	3	11258	7·49	1·34	4·23	2·4		
East Crinnis, Hudson's	30 Nov. 9 28 A.M. to 1 Dec. 7 55 A.M.	34	3005	$\frac{1}{21}$	1	12	5	4717	3·5	1·7	4·17	11·2		

The engines were taken without any previous preparation, and they were worked without intermission, at a speed just sufficient to keep the mines clear from water; but without permitting the pumps to draw air (*go in fork*). The workmen exercised their own discretion in the mode of working; for I purposely abstained from any other interference with them than was sufficient to satisfy myself that every thing was exposed to my notice, and fairly and honestly performed.

* The lengths of the *lifts* and of the rods do not coincide, because the former overlap each other in every case, in order that the higher pumps may draw out of the same cisterns into which the lower empty; and because the rods which take the different *lifts* are also doubled at the *sets-off*.

The results will appear in

TABLE V.

Mines and Engines.	Weight of the bushel of Coal.		Duty (in lbs. lifted one foot high) performed by each bushel of Coal*.		
	As taken from the heap.	When dry.	Bushel measured.	84 lbs. as taken from the heap.	84 lbs. dry.
	lbs.	lbs.			
Huel Towan, Wilson's	100	93·8	86,585,079	72,687,853	77,533,710
Binner Downs, Swan's	92·6	83·4	73,877,810	66,956,572	74,395,923
East Crinnis, Hudson's	88·3	84·1	73,954,606	70,003,555	73,502,699

III.—*The work accomplished for a certain expense.*—The foregoing details supply all that is requisite for this inquiry, except the prices of the materials consumed; these were coal, at the rate of forty-one shillings for 72 measured bushels †; grease, forty-five shillings and sixpence per 112 lbs.; and oil, four shillings and twopence per gallon; at which rates the results were by Huel Towan, Wilson's engine, 1085 tons; Binner Downs, Swan's engine, 1006 tons; East Crinnis, Hudson's engine, 870 tons; lifted one foot high for the expense of one farthing.

As supplementary to the general object of the first part of this inquiry, it may be useful to compare the maxima of pressures which obtain in the cylinders, with known elasticities in the boilers; the loads of the engines remaining unchanged.

TABLE VI.—*Load of engines, and relative pressures of steam in the boilers and cylinders.*

Mines and Engines.	Load on the Piston, in lbs. per square inch of its area.	Pressure of steam in lbs. per square inch.	
		In the Boiler.	In the Cylinder§.
Huel Towan, Wilson's‡	10·2	61·8	27
Binner Downs, Swan's	10·23	74·78 58	26 25
——— Burn's	10·7	55	30·5
East Crinnis, Hudson's	11·4	36·8 26·3	25 21
Huel Vor, Trelawny's	14·7	47	30·5
———, Borlase's	12·1	40	30·5

* These numbers are on the assumption that each pump delivers the full computed quantity: but in an experiment at Huel Towan, made by Sir John Rennie and myself, the *actual* compared with the calculated delivery was as 0·924 to unity. I have repeated the comparison at the same place, with a similar result.

† The bushel measure with a heaped *head* is the same which was used in Mr. Watt's time, varying only as prescribed by law.

‡ The figure in p. 482 refers to this engine.

§ All the pressures mentioned throughout this paper are absolute, and as if acting against a vacuum.

Many subjects which are yet undetermined have pressed on my attention during these experiments, among which the *steam-case* and *air-pump* are not the least important.

If any condensation take place in the case, when protected from the influence of the external air, it must be by radiation to the rarer steam within the cylinder. Now such influence, if exerted during at least two-thirds of every stroke *, would not only not increase the force of the engine by adding to the elasticity of the steam, but would render requisite the injection of a larger quantity of cold water into the condenser to effect condensation, and thereby add to the burden of the air-pump †.

There must be a point at which the resistance of vapour, not abstracted, to the descent of the piston, and the pressure of the atmosphere on the air-pump whilst discharging its load, are at a minimum. Beyond this, if it be attempted to reduce the force of the vapour, by injecting more cold water, the burden of the air-pump is increased by the exposure of its piston to the atmosphere for a longer time during its discharge; whilst on the other hand, if it be sought to lessen the duration of atmospheric pressure on the air-pump, by diminishing the quantity of cold water introduced into the condenser, the increased elasticity of the unabstracted vapour offers a greater resistance to the descent of the piston ‡.

This subject presents many inviting topics of inquiry; but the pursuit of them, and the earlier preparation of the details § which I have now the honour to submit to the Institution, have been prevented by more pressing occupations.

4, Clarence Street, Penzance, W. J. HENWOOD.
August 30th, 1837.

* See Table IV.

† Brewster's Edinburgh Journal of Science, O. S. IX., p. 162.

‡ Ibid., X. p. 40.

§ A short notice of these experiments appeared in Brewster's Edinburgh Journal of Science, N. S., VI. p. 246.

LXXII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 369.]

April 11.—A paper was read, entitled, " On a new equi-atomic compound of Bicyanide with Binoxide of Mercury." By James F. W. Johnston, Esq., F.R.S.

In this paper an account is given of the properties of a salt, obtained by agitating with red oxide of mercury a small proportion of hydrocyanic acid, and which the author finds to be distinguished from the bicyanide of mercury by its sparing solubility in cold water, by the strong alkaline reaction exhibited by its solution, (a property which indicates an excess of mercury,) and by its susceptibility of detonation by heat, depending on this excess being in the state of an oxide, and on the action of the oxygen on a portion of the carbon of the cyanogen it contains, and the presence of which is shown by the disengagement of hydrocyanic acid gas when acted on by hydrosulphuric and hydrochloric acids.

The analysis of this salt, given by the author, shows it to consist of

Carbon..........	5·203
Nitrogen........	6·025
Oxygen	3·098
Mercury	85·674
	100.

The formula of which composition is as follows:

$$\text{Hg. Cy.}_2 + \text{Hg. O.}_2.$$

April 18.—The following papers were read, viz.—

" On the Constitution of the Resins." *Part I.* By James F. W. Johnston, Esq., F.R.S.*

The object of the general investigation, of which the commencement is given in this paper, is to determine the relative composition of the various resins which occur in nature, and to trace the analogies they exhibit in their constitution; and also to ascertain how far they may be regarded as being derived from one common principle, and whether they admit of being all represented by one or more general formulæ.

The chemical investigation of the resin of mastic shows that this substance consists of two resins; the one soluble, and acid; the other insoluble, and having no acid properties. The formulæ expressing the analysis of each of these are given by the author. He also shows that a series of analyses may be obtained which do not indicate the true constitution of a resin. The soluble resin, when exposed to the prolonged action of a heat exceeding 300° Fahr. is partly converted into a resin containing three, and partly into one containing five equivalent parts of oxygen, the proportion of carbon remaining constant. The same resin combines with bases, so as to

* See our present volume, p. 340.

form four series of salts; which, in the case of oxide of lead, consist of equivalents of resin and of oxide in the proportions, respectively, of two to one; three to two; one to one; and one to two. This soluble resin in combining with bases does not part with any of its oxygen; but if any change takes place in its constitution, it consists in the hydrogen being replaced by an equivalent proportion of a metal; and formulæ are given representing the salts of lead on this theoretical view. By boiling the resin in contact with ammonia and nitrate of silver, or perhaps with nitrate of ammonia, it is converted into a resin which forms a bisalt with oxide of silver, in which there is also an apparent replacement of hydrogen by silver.

The resin next examined is that of dragon's blood: and the conclusions deduced from its analysis are the following. First, that the lump dragon's blood is the natural and pure resin, while the strained and red varieties, being manufactured articles, are more or less decomposed: secondly, that this resin retains alcohol and ether, as most other resins do, with considerable tenacity; but that these solvents may be entirely expelled by a long-continued exposure to a temperature not higher than 200° Fahr.: and lastly, the formulæ representing its chemical composition is given.

"Researches in Embryology." — *Second Series.* By Martin Barry, M.D., F.R.S.E., Fellow of the Royal College of Physicians in Edinburgh. Communicated by P. M. Roget, M.D. Sec. R.S.*

The author having, in the first series of these researches, investigated the formation of the mammiferous ovum, describes in this second series its incipient developement. The knowledge at present supposed to be possessed of the early stages in the developement of that ovum, consists chiefly of inferences from observations made on the ovum of the bird.

But there exists a period in the history of the ovum of the mammal, regarding which we have hitherto scarcely any direct or positive knowledge. It appeared, therefore, highly desirable to obtain a series of observations in continuous succession on the earliest stages of developement. In conducting this investigation, the author purposely confined his attention to a single species, namely, the rabbit, of which he examined more than a hundred individual animals. Besides ova met with in the ovary, apparently impregnated, and destined to be discharged from that organ, he has seen upwards of three hundred ova in the Fallopian tube and uterus; very few of the latter exceeding half a line in their diameter. The results of these investigations have compelled the author to express his dissent from some of the leading doctrines of embryology, which at present prevail, as respects not only the class Mammalia, but the animal kingdom at large. The following are the principal facts which the author has observed in the developement of the mammiferous ovum.

The difference between the mature and immature ovum consists in the condition of the yelk; the yelk of the mature ovum containing no oil-like globules. Both maceration and incipient absorption

* An abstract of Dr. Barry's First Series of Researches in Embryology will be found in L. & E. Phil. Mag., vol. xiii. p. 458.

produce changes in the unimpregnated ovum, which in some respects resemble those referable to impregnation. During the rut, the number of Graafian vesicles appearing to become prepared for discharging their ova, exceeds the number of those which actually discharge them. Ova of the rabbit which are destined to be developed, are in most instances discharged from the ovary in the course of nine or ten hours *post coitum*; and they are all discharged about the same time.

There is no condition of the ovum uniform in all respects which can be pointed out as the particular state in which it is discharged from the ovary; but its condition is in several respects very different from that of the mature ovum *ante coitum*. Among the changes occurring in the ovum before it leaves the ovary, are the following: viz. the germinal spot, previously on the inner surface, passes to the centre of the germinal vesicle; the germinal vesicle, previously at the surface, returns to the centre of the yelk; and the membrane investing the yelk, previously extremely thin, suddenly thickens. Such changes render it highly probable that the ovary is the usual seat of impregnation. The author considers this view as being not incompatible with the doctrine that contact between the seminal fluid and the ovum is essential to impregnation, since he has found, in the course of his researches, that spermatozoa penetrate as far as to the surface of the ovary. The retinacula and tunica granulosa are the parts acted upon by the *vis a tergo*, which expels the ovum from the ovary. These parts are discharged with the ovum, render its escape gradual, probably facilitate its passage into the Fallopian tube, and appear to be the bearers of fluid for the immediate imbibition of the ovum. After the discharge of the ovum from the ovary, the ovisac is obtainable free from the vascular covering, which, together with the ovisac, had constituted the Graafian vesicle. It is the vascular covering of the ovisac which becomes the corpus luteum. Many ova, both mature and immature, disappear at this time by absorption. In some animals minute ovisacs are found in the infundibulum, the discharge of which from the ovary appears referable to the rupture of large Graafian vesicles, in the parietes or neighbourhood of which those ovisacs had been situated.

The diameter of the rabbit's ovum, when it leaves the ovary, does not generally exceed the 135th part of an inch, and in some instances it is still smaller. The ovum enters the uterus in a state very different from that in which it leaves the ovary; hence the opinion, that "in their passage through the tube the ova of Mammalia undergo scarcely any metamorphosis at all," is erroneous. Among the changes taking place in the ovum during its passage through the Fallopian tube are the following; viz. 1. An outer membrane, the chorion, becomes visible. 2. The membrane originally investing the yelk, which had suddenly thickened, disappears by liquefaction; so that the yelk is now immediately surrounded by the thick transparent membrane of the ovarian ovum. 3. In the centre of the yelk, that is, in the situation to which the germinal vesicle returned before the ovum left the ovary, there arise several very large and exceedingly transparent vesicles: these disappear,

and are succeeded by a smaller and more numerous set; several sets thus successively come into view, the vesicles of each succeeding set being smaller than the last, until a mulberry-like structure has been produced, which occupies the centre of the ovum. Each of the vesicles of which the surface of the mulberry-like structure is composed contains a pellucid nucleus; and each nucleus presents a nucleolus.

In the uterus a layer of vesicles of the same kind as those of the last and smallest set here mentioned makes its appearance on the whole of the inner surface of the membrane which now invests the yelk. The mulberry-like structure then passes from the centre of the yelk to a certain part of that layer, (the vesicles of the latter coalescing with those of the former where the two sets are in contact to form a membrane,) and the interior of the mulberry-like structure is now seen to be occupied by a large vesicle containing a fluid and granules. In the centre of this vesicle is a spherical body having a granulous appearance, and containing a cavity apparently filled with a colourless and pellucid fluid. This hollow spherical body seems to be the true germ. The vesicle containing it disappears, and in its place is seen an elliptical depression filled with a pellucid fluid. In the centre of this depression is the germ, still presenting the appearance of a hollow sphere. The germ separates into a central and a peripheral portion, the central portion occupies the situation of the future brain, and soon presents a pointed process which is the rudiment of the spinal cord. These parts at first appearing granulous are subsequently found to consist of vesicles.

Thus the central portion of the nervous system is not originally a fluid contained within a tube, but developes itself in a solid form before any other part. The central portion of the nervous system sometimes attains a considerable degree of developement, although it be exceedingly minute; thus an instance has been met with in which the developement of this part had reached a stage scarcely inferior to that in another instance, in which the corresponding part measured more than ten times the length.

There does not occur in the mammiferous ovum any such phenomenon as the "splitting" of a membrane into the so-called "serous, vascular, and mucous laminæ." Rathke had already found that parts previously supposed by Baer and others to be formed by the so-called "germinal membrane," really originate independently of it: these parts are the ribs, pelvic bones, and the muscles of the thorax and abdomen, which according to Rathke arise in a part proceeding out of the "primitive trace" itself. Reichert had previously discovered that the part originating the lower jaw and hyoid bone "grows out of the primitive trace." The author beginning with an earlier period goes farther than these observers, and shows that the so-called "primitive trace" itself does not arise in the substance of a membrane, but presents a comparatively advanced stage of the object above described as the true germ. Hence the author suggests, there is no structure entitled to be denominated the "germinal membrane."

The most important of the foregoing facts respecting the developement of the mammiferous ovum, however opposed they may be to received opinions, are in accordance with, and may even explain, many observations which have been made on the developement of other animals as recorded in the delineations of preceding observers. If in the ovum of the bird the germinal vesicle in like manner returns to the centre of the yelk, the canal and cavity known to exist in the yelk of that ovum might be thus explained. The ovum may pass through at least one-and-twenty stages of developement, and contain, besides the embryo, four membranes, one of which has two laminæ, before it has itself attained the diameter of half a line, a fifth membrane having disappeared by liquefaction within the ovum.

The size of the minute ovum in the Fallopian tube and uterus affords no criterion of the degree of its developement; nor do any two parts of the minute ovum, in their developement, necessarily keep pace with one another.

The proportion of ova met with in these researches, which seemed to be abortive, has amounted to nearly one in eight. Sometimes two yelk-balls exist in the same ovum. With slight pressure, the ovum, originally globular, becomes elliptical. Its tendency to assume the latter form exists especially in the chorion, and seems to be in proportion to its size.

The author has discovered that when the germinal vesicle is first seen it is closely invested by an extremely delicate membrane. This membrane subsequently expanding is that in which the yelk is formed. He has traced the chorion from stage to stage up to the period when it becomes villous, and shows that it is not, as he formerly supposed, the thick transparent membrane itself of the ovarian ovum, but a thin envelope closely investing that membrane, and not appreciable as a distinct structure until the ovum has been crushed. When the chorion first admits of demonstration as a distinct structure the ovum consists of three membranes, a state which the author has seen in an ovum no farther advanced than about an inch into the Fallopian tube. The chorion subsequently thickens and imbibes a quantity of fluid presenting a gelatinous appearance.

April 25.—A paper was in part read, entitled, "Account of Experiments on Iron-built Ships, instituted for the purpose of discovering a Correction for the Deviation of the Compass produced by the Iron of Ships." By George Biddell Airy, Esq., M.A., F.R.S., A.R.

May 2.—A paper was in part read, entitled, "On the Motion of the Blood." By James Carson, M.D., F.R.S.

May 9.—The reading of a paper, entitled, "On the Motion of the Blood." By James Carson, M.D., F.R.S., was resumed and concluded.

After referring to his paper contained in the Philosophical Transactions for 1820, relative to the influence of the elasticity of the lungs as a power contributing to the effectual expansion of the heart, and promoting the motion of the blood in the veins, the author states that his object in this paper is to explain more fully the mode in which these effects are produced, and to corroborate by additional facts and observations the arguments adduced in its support. He

endeavours, from a review of the circumstances under which the veins are placed, to show the inconclusiveness of the objections which have been urged by various physiologists against his and the late Sir David Barry's theory of suction; namely, that the sides of a pliant vessel, when a force of suction is applied, will collapse and arrest the further transmission of fluid through that channel. The considerations which he deems adequate to give efficacy to the power of suction in the veins of a living animal are, first, the position of the veins by which, though pliant vessels, they acquire in some degree the properties of rigid tubes; secondly, the immersion of the venous blood in a medium of a specific gravity at least equal to its own; thirdly, the constant introduction of recrementitious matter into the venous system at its capillary extremities by which the volume of the venous blood is increased, and its motion urged onwards to the heart in distended vessels; and lastly, the gravity of the fluid itself, creating an outward pressure at all parts of the veins below the highest level of the venous system. The author illustrates his positions by the different quantities of blood which are found to flow from the divided vessels of an ox, according to the different modes in which the animal is slaughtered.

The reading of a paper, entitled, "Account of Experiments on Iron-built Ships, instituted for the purpose of discovering a Correction for the Deviation of the Compass produced by the Iron of the Ships." By George Biddell Airy, Esq., A.M., F.R.S., Astronomer Royal, was also resumed and concluded.

In this paper the problem of the deviation of a ship's compass, arising from the influence of the iron in the ship, more particularly in iron-built ships, is fully investigated; and the principles on which the correction for this deviation depends having been determined, practical methods for neutralizing the deviating forces are deduced and illustrated by experimental application. The author states that, for the purpose of ascertaining the laws of the deviation of the compass in the iron-built steam-ship the Rainbow, four stations were selected in that vessel, about four feet above the deck, and at these the deviations of the horizontal compasses were determined in the various positions of the ship's head. All these stations were in the vertical plane, passing through the ship's keel, three being in the after part of the ship and one near the bow. Observations were also made for determining the horizontal intensity at each of the stations. The deviations of dipping needles at three of these stations were also determined, when the plane of vibration coincided with that of the ship's keel, and also when at right angles to it.

After describing the particular method of observing rendered necessary by the nature of the vessel and the circumstances of her position, the author gives the disturbance of the horizontal compass at the four stations deduced from the observations. The most striking features in these results are, the very great apparent change in the direction of the ship's head, as indicated by the compass nearest the stern, corresponding to a small real change in one particular position, the former change being 97°, whereas the latter was only

23°, and the small amount of disturbance indicated by the compass near the bow.

After giving the observations for the determination of the influence of the ship on the horizontal intensity of a needle suspended at each of the stations, in four different positions of the ship's head, and the disturbances of the dipping needle at three of these stations, the author enters upon the theoretical investigation.

The fundamental supposition of the theory of induced magnetism, on which Mr. Airy states his calculations to rest, is, that, by the action of terrestrial magnetism, every particle of iron is converted into a magnet, whose direction is parallel to that of the dipping needle, and whose intensity is proportional to that of terrestrial magnetism, the upper end having the property of attracting the north end of the needle, and the lower end that of repelling it.

The attractive and repulsive forces of a particle on the north end of the needle, in the directions of rectangular axes towards north, towards east, and vertically downwards, and of which the compass is taken as the origin, are first determined on this supposition in terms of the co-ordinates; and thence the true disturbing forces of the particle in these directions. The disturbing forces produced by the whole of the iron of the ship are the sums of the expressions for every particle. Expressing this summation by the letter S, and transforming the rectangular into polar co-ordinates, Mr. Airy gives to the expressions for the disturbing forces the simplifications which they admit of, on the supposition that the compass is in the vertical plane passing through the ship's keel, and that the iron is symmetrically disposed on both sides of that plane. He thus deduces for the disturbing forces acting on the north or marked end of the needle,

$-\mathrm{I}\cos\delta.\,\mathrm{M} + \mathrm{I}\cos\delta.\,\mathrm{P}\cos 2\,\mathrm{A} + \mathrm{I}\sin\delta.\,\mathrm{N}\cos\mathrm{A}$, towards the magnetic north;

$\mathrm{I}\cos\delta.\,\mathrm{P}\sin 2\,\mathrm{A} + \mathrm{I}\sin\delta.\,\mathrm{N}\sin\mathrm{A}$, towards magnetic east;

$-\ \mathrm{I}\sin\delta.\,\mathrm{Q} + \mathrm{I}\cos\delta.\,\mathrm{N}\cos\mathrm{A}$, vertically downwards:

Where I represents the intensity of terrestrial magnetism; δ the dip; A the azimuth of the ship's head; and M, N, P, Q, constants depending solely on the construction of the ship, and not changing with any variations of terrestrial localities or magnetic dip or intensity.

From the consideration of these expressions for the disturbing forces is deduced the following simple rule for the correction of a compass disturbed by the induced magnetism only of the iron in a ship.

1. Determine the position of Barlow's plate with regard to the compass, which will produce the same effect as the iron in the ship.

2. Fix Barlow's plate at the distance and depression determined by the last experiment, but in the opposite azimuth.

3. Mount another mass of iron at the same level as the compass, but on the starboard or larboard side, and determine its position so that the compass points correctly when the ship's head is N.E., S.E.,

S.W. or N.W.; then the compass will be correct in all positions of the ship's head, and in all magnetic latitudes.

When the disturbing iron of the ship is at the same level as the compass, the correction is stated to be much more simple, it being then only necessary to introduce a single mass of iron at the starboard or larboard side, and at the same level as the compass.

It is farther remarked that if one mass of iron is placed exactly opposite another equal mass, both in azimuth and in elevation, it doubles its disturbing effect: if one mass be placed opposite the other in azimuth, but with elevation instead of depression, or *vice versâ*, it destroys that term of the disturbance which depends on sin A, and doubles that which depends on sin 2 A: and if one mass be placed at the same level as the compass, its effects may be destroyed by placing another mass at the same level, in an azimuth differing 90° on either side. If a disturbance, from whatever cause arising, follow the law of + sin 2 A, (changing sign in the successive quadrants, and positive when the ship's head is between N. and E.), it may be destroyed by placing a mass of iron on the starboard or larboard side at the same level as the compass; if it follow the law of − sin 2 A, the mass of iron must be on the fore or aft side.

From the consideration of the expression for the disturbing forces produced by the ship, it is farther inferred, that both in the construction of the ship and in the fixing of correctors, no large mass of iron should be placed below the compass.

The expressions for the disturbing forces towards north and east, being transformed into forces towards the ship's head and towards the starboard side, give

$I \cos\delta . (-M + P) \cos A + I \sin\delta . N$, for the former, and

$I \cos\delta . (M + P)$, for the latter.

The author next proceeds to investigate the effects which result from the combination of induced magnetism with permanent magnetism. Calling H, S and V the new forces arising from the latter, and directed towards the ship's head, its starboard side, and vertically downwards, the whole disturbing force towards the ship's head becomes

$$H + I \cos\delta . (-M + P) \cos A + I \sin\delta . N;$$

and the whole disturbing force towards the starboard side,

$$S + I \cos\delta . (M + P) \sin A.$$

The manner in which the numerical values of these quantities may be found from experiment is then pointed out, and being determined from the observations on board the Rainbow, at Station I., a comparison is made between the observed disturbances of the needles, and those which would result from the action of the ship as a permanent magnet. From this comparison it appears that almost the whole disturbance is accounted for by the permanent magnetism, and that the residual part follows with sufficient approximation the law of changing signs at the successive quadrants. For the complete verification of the theory it remained only to effect an actual correction of the compass. This was done by placing below the compass, in a

position determined by the previously-ascertained numerical values, a large bar magnet to neutralize the effects of the permanent magnetism of the ship, and a roll of soft iron on one side of the compass to counteract the disturbance arising from induced magnetism. That this correction was effective appears from the very small amount of uncorrected disturbance then observed in the compass.

The observations of the compasses at stations II., III., IV., are similarly discussed: the disturbing force arising from the permanent magnetism of the ship being in like manner determined, a comparison is instituted between the observed and computed disturbance of the compass; and the results of this comparison, with the exception of the observations at Station IV., are found to be in perfect accordance with the theory. Attempts are made to correct the compasses at these stations in the same manner as at Station I., but owing to the imperfection of the compasses they did not succeed so perfectly.

The observations made with the dipping needle are next discussed, and the values of the constants are deduced from them. The general agreement of those determined from the observations when the needle vibrated in the direction of the ship's keel, with those deduced from the observations when the needle vibrated transversely, is pointed out, and is considered an additional proof of the general correctness of the theory.

Observations on the disturbance of the compass in the iron-built sailing-ship Ironsides are next described. These are similar to those in the Rainbow, but not so extensive; and they are discussed on the same principles. From this discussion it is considered that the theory is in perfect accordance with the facts observed both with regard to the deviations and the intensities. The correction of one compass was effected by a tentative process, which the author considers likely to be of the highest value in the correction of the compasses of iron-ships in general. The ship's head being placed exactly north, as ascertained by a shore compass, a magnet was placed upon the beam from which the compass was suspended, with the direction of its length exactly transverse to the ship's keel: it was moved upon the beam to various distances till the compass pointed correctly, and then it was fixed. Then the ship's head was placed exactly east, and another magnet, with its length parallel to the ship's keel, was placed upon the same beam, and moved to different distances till the compass pointed correctly, and then it was fixed. The correction for induced magnetism was neglected, but there would have been no difficulty in adjusting it by the same process, placing the vessel's head in azimuth 45° or 135° or 225° or 315°.

In conclusion Mr. Airy makes the following remarks:—

The deviations of the compass at four stations in the Rainbow, and at two stations in the Ironsides, are caused by two modifications of magnetic power; the one being the independent magnetism of the ship, which retains, in all positions of the ship, the same magnitude and the same direction relatively to the ship; the other being the induced magnetism, of which the force varies in magnitude and direction when the ship's position is changed. In the instances

mentioned, the effect of the former force was found greatly to exceed that of the latter.

It appears that experiments and observations similar to those applied in the above cases are sufficient to obtain with accuracy the constants on which at any one place the ship's action on the horizontal needle depends, namely,

$$\frac{H}{I\cos\delta}+\tan\delta.N,\ \frac{S}{I\cos\delta},\ M,\ \text{and}\ P;$$

and that by placing a magnet so that its action shall take place in a direction opposite to that which the investigations show to be the direction of the ship's independent magnetic action, and at such a distance that its effect is equal to that of the ship's independent magnetism, and by counteracting the effect of the induced magnetism by means of the induced magnetism of another mass, according to rules which are given, the compass may be made to point exactly as if it were free from disturbance.

It appears also, that by an easy tentative method, the compass may now be corrected without the labour of any numerical investigations or any experiments except those of merely making the trials. Although the uniformity of the induced magnetism under similar circumstances is to be presumed, yet the invariability of the independent magnetism during the course of many years is by no means certain.

These statements suggest the following as rules which it is desirable to observe in the present infancy of iron-ship building. It appears desirable that

1. Every iron sea-going ship should be examined by a competent person for the accurate determination of the four constants above-mentioned for each of the compasses of the ship, and a careful record of these determinations should be preserved as a magnetic register of the ship.

2. The same person should be employed to examine the vessel at different times, with the view of ascertaining whether either of the constants changes in the course of time.

3. In the case of vessels going to different magnetic latitudes, the same person should make arrangements for the examination of the compasses in other places with a view to the determination of the constant N.

4. The same person should examine and register the general construction of the ship, the position and circumstances of her building, &c., with a view to ascertain how far the values of the magnetic constants depend on these circumstances, and in particular to ascertain their connexion with the value of the prejudicial constant M.

5. The same person should see to the proper application of the correctors and the proper measures for preserving the permanency of their magnetism.

The most remarkable result in a scientific view from the experiments detailed in the present paper is, the great intensity of the permanent magnetism of the malleable iron of which the ship is composed.

May 16.—A paper was read, entitled "On the visibility of certain rays beyond the ordinary red rays of the Solar Spectrum." By J. S. Cooper, Esq., in a letter to Michael Faraday, Esq., D.C.L., F.R.S., &c., &c. Communicated by Dr. Faraday.

The author states his having observed an extension of the red portion of the solar spectrum, obtained in the ordinary way, beyond the space it occupies when seen by the naked eye, by viewing it through a piece of deep blue cobalt glass. He finds that the part of the spectrum thus rendered perceptible to the right is crossed by two or more very broad lines or bands: and observes that the space occupied by the most powerful calorific rays, coincides with the situation of the red rays thus rendered visible by transmission through a blue medium. The author expresses a regret that he has not had sufficient leisure to pursue the investigation of these phænomena.

A paper was also in part read, entitled, "Fifth letter on Voltaic Combinations, with some account of the effects of a large constant Battery:" addressed to Michael Faraday, Esq., D.C.L., F.R.S., &c. By John F. Daniell, Esq., F.R.S.

The Society then adjourned over the Whitsun Recess, to meet again on the 30th of May.

GEOLOGICAL SOCIETY.

President's Anniversary Address, Feb. 15: *continued from p.* 461.

GEOLOGICAL DYNAMICS.

In that part of geology which I have termed Geological Dynamics, and which investigates and applies those causes of change by which we may hope to explain geological phenomena, we may still observe that fundamental antithesis of opinion which has long existed on the subject;—the division of our geological speculators into *Catastrophists* and *Uniformitarians*;—into those who read in the rocks of the globe the evidence of vast revolutions, of an order different from any which those of man has survived;—and those who see in the condition of the earth the result of a series of changes which are still going on without decay, the same powers which produced the existing valleys and mountains being yet at work about us. Both these opinions have received their contributions during the preceding year: Mr. Darwin having laid before us his views of the formation of mountain chains and volcanos, which he conceives to be the effect of a gradual, small, and occasional elevation of continental masses of the earth's crust*; while Mr. Murchison gathers from the researches in which he has been engaged, the belief of a former state of paroxysmal turbulence, of much deeper rooted intensity and wider range than any that are to be found in our own period; and M. de Beaumont, in France, has endeavoured to prove that Etna and many other mountains must have been produced by some gigantic and extraordinary convulsion of the earth. Both Mr. Darwin and M. de

* An abstract of Mr. Darwin's paper was given in L. & E. Phil. Mag., vol. xii. p. 584.

Beaumont refer to the same examples; and while M. de Beaumont conceives that the cones of the Andes must have been formed by an abrupt elevation, caused by subterranean force, Mr. Darwin has maintained the opinion, that these lofty summits have been gradually thrust into the place which they occupy by a series of successive injections of molten matter from below, each intruded portion of fluid having time to harden into rock before it was burst and again injected by the next molten mass. For how otherwise, he asks, can we conceive the strata to be thrust into a vertical position by a liquid from below, without the very bowels of the earth gushing out? Without attempting to answer this question, we may observe, that when we suppose, as Mr. Darwin supposes, a vast portion of the earth's crust, the whole territory of Chili for example, to rest on a lake of molten stone, there is considerable force in M. de Beaumont's argument:—that when such a fluid is raised to the top of a mountain ten or twenty thousand feet high, the pressure upon the crust which is in contact with the fluid must be more than a thousand atmospheres; and who, *he* too asks, flatters himself that he knows enough of the interior machinery of volcanos, to be certain that this vast pressure, acting upon a large surface, may not, by some derangement of its safety-valve, the volcanic vent, produce effects to which we cannot assign any limit?

In speaking of Mr. Darwin's researches I cannot refrain from expressing for myself, and I am sure I may add for you, our disappointment and regret that the publication of Mr. Darwin's journal has not yet taken place. Knowing, as we do, that this journal contains many valuable contributions to science, we cannot help lamenting, that the customs of the Service by which the survey was conducted have not yet allowed this portion of the account of its results to be given to the world.

Although not communicated to us, but to our Alma Mater the Royal Society, I may notice Mr. Hopkins's endeavours to throw light upon such subjects as this by the aid of mathematical reasoning. The researches of Mr. Hopkins respecting the effects which a force from below would produce upon a portion of the earth's crust, have already interested you, and would be of still greater value if the directions of faults and fissures which result from his theory did not depend very much upon that which in most cases we cannot expect to know, the form of the area subjected to such strain. Mr. Hopkins has since been employing himself in tracing the consequences of another idea, truly ingenious and philosophical, and which a person in full possession of the resources of mathematics could alone deal with. Some of the effects which the sun and moon produce upon the earth (as the precession and nutation,) include the attraction of those bodies upon the interior portion of the earth, and have hitherto been deduced from the theory by mathematicians, upon the supposition that the earth is solid. But what if the central portion of the earth were fluid? What if it appeared, by calculation, that the fluid internal condition would make the amount of the precession of the equinoxes, or of the nutation of the axis, different

from that which the solid spheroid would give? What if it appeared that the precession and nutation thus calculated for a fluid interior agreed better with observation than the result hitherto obtained by supposing the earth solid? If this were so, we should have evidence of the earth's interior fluidity, evidence, too, of a perfectly novel and most striking nature. But to answer these questions is far from an easy task; the precession of the solid earth is a problem in which Newton erred, and in which the greatest mathematicians of modern times have not found their greatest strength superfluous. Yet how incomparably more difficult in all cases is the mechanics of fluid than of solid bodies! It may, therefore, require more than one trial before any satisfactory solution of the problem can be obtained. Mr. Hopkins has attacked it by the aid of certain hypotheses, and the result is, so far, not favourable to the decisiveness of this test of the interior condition of the earth; but notwithstanding this state of things, I venture to say on your behalf, Gentlemen, that an idea so full of promise of that which we so much desire, and which seems to be so utterly out of our reach, the knowledge of the condition of the centre of the earth,—that such an idea is not to be lightly abandoned*.

M. Necker, of Geneva, offered an addition to the causes of convulsions of the earth, which are contemplated by our Geological Dynamics, in a paper in which he ascribed the earthquakes which took place in the southern provinces of Spain, in 1829, to the falling in of strata, the subjacent gypseous and saliferous masses being washed out by subterraneous currents†. Without denying all influence to such a cause, we may observe that it does not appear likely that

* The following are the results at which Mr. Hopkins has arrived, supposing the earth to consist of a homogeneous spheroidal shell filled with a fluid mass of the same density as the shell:—

1. The precession will be the same, whatever be the thickness of the shell, as if the whole earth were solid.

2. The lunar nutation will be the same as for the solid spheroid, to such a degree of approximation, that the difference would be inappreciable to observation.

3. The solar nutation will be sensibly the same as for the solid spheroid; unless the thickness of the shell be very nearly of a certain value, something less than one fourth the earth's radius, in which case this nutation might become much greater than for the solid spheroid.

4. In addition to the above motions of precession and nutation, the pole of the earth would have a small circular motion, depending entirely on the internal fluidity. The radius of the circle thus described would be the greatest when the thickness of the shell should be least; but the inequality thus produced would not, for the smallest thickness of the shell, exceed a quantity of the same order as the solar nutation; and for any but the most inconsiderable thickness of the shell, would be entirely inappreciable to observation.

Mr. Hopkins intends hereafter to consider the case of variable density. [See our present volume, p. 364.—Edit.]

[† An abstract of M. Necker's paper has appeared in the present volume, p. 370.—Edit.]

there would be thus produced, simultaneously, any greater effects than those which are known to have occurred from the falling in of unsupported mines; and these have never approached in their scale to any except the smallest earthquakes.

While geologists are thus looking in all directions for causes which may produce the phenomena which they study, it is natural that the powerful, but as yet mysterious influences of electricity should draw their attention. Mr. Robert Were Fox has endeavoured to show, that by voltaic agency, a laminated structure, and deposits of metal in cracks, resembling metallic veins, may be produced in masses of clay. The experiments are of an interesting kind, and it can hardly be doubted that voltaic agency had some influence in such cases as those described by Mr. Fox; although Mr. Henwood and Mr. Sturgeon have failed in attempting to reproduce his results, and although results much resembling these occur in cases where no electrical action is suspected. But we may remark that the conditions under which such voltaic effects are produced have not yet been attempted to be defined with any accuracy; and that till this is done, the reality of such agency can neither be verified nor applied to geological speculations.

A reflection which naturally offers itself upon this review of our recent career, is this:—that different portions of the science of geology advance with very different rapidity. Descriptive Geology is constantly and actively progressive: facts are accumulated by observers in every land; and though facts are, in truth, of no value, at least for any purpose of science, except so far as they are reduced to some classification, yet on the other hand, sound classifications are perpetually, almost necessarily, suggested, when observation is vigilant and persevering. Even if we at first express our facts in terms of a false classification, we find afterwards the means of translating them into the language of a true one. And the spirit of geological observation is so widely diffused, and so thoroughly roused, that I trust we need not anticipate any pause or retardation in the career of Descriptive Geology. I confess, indeed, for my own part, I do not look to see the exertions of the present race of geologists surpassed by any who may succeed them. The great geological theorizers of the past belong to the *Fabulous Period* of the science; but I consider the eminent men by whom I am surrounded as the *Heroic Age* of geology. They have slain its monsters, and cleared its wildernesses, and founded here and there a great metropolis, the queen of future empires. They have exerted combinations of talents which we cannot hope to see often again exhibited, especially when the condition of the science which produced them is changed. I consider that it is now the destiny of geology to pass from the heroic to the *Historical Period.* She can no longer look for supernatural successes, but she is entering upon a career, I trust a long and prosperous one, in which she must carry her vigilance into every province of her territory, and extend her dominion over the earth, till it becomes, far more truly than any before, an universal empire.

Such are the prospects of Descriptive Geology;—of the geology of facts and classifications. To our knowledge of causes we can look with no such certainty of its progress being steady and rapid; or rather, we are certain that the advance must be slow, and may be often and long interrupted. For it is not an advance, to suggest one or another hypothetical cause of change, without assigning the laws and amount of the change: it is hardly an advance even to calculate the results of our hypotheses on assumed conditions. To obtain by induction, from adequate facts, the laws of change of the organic and inorganic creation,—this alone can lead us to those discoveries which must form the epochs of Geological Dynamics. And we have yet to learn, whether man's past duration upon the earth, whether even that which is still destined to him, is such as to allow him to philosophize with success in such matters;—whether, not individuals only, not a generation alone, but whether the whole species be not too ephemeral, to penetrate, by the unassisted powers of its reason, into the mystery of its origin:—whether man, placed for a few centuries on the earth as in a school-room, have time to strip the wall of its coating, and count its stones, before his Parent removes him to some other destination.

And now, Gentlemen, I approach the close of my task, and of the office which has imposed it upon me; an office which has been to me a source of unmingled gratification. The good opinion implied by your selection of me, the good opinion of such a body of men, was an occasion of sincere and earnest self-congratulation,—a self-congratulation hardly damped by my consciousness of an imperfect acquaintance with your science;—since I trusted that you, though not unaware of my defects, had judged that good will, and a disposition to look at the subject in its largest aspect, might in some measure compensate for them. And if I needed other grounds of satisfaction in the employment which I am thus bringing to its close, I might find them in the reflections I have just been led to make in the progress and prospects of the science with which you are concerned. For it has ever been one of my most cherished occupations, and will, I trust, long be so, to trace the principles and laws by which the progress of human knowledge is regulated from age to age in each of its provinces. To have had brought familiarly under my notice, in a living form, the daily advance of a science so large and varied as yours, has been, as it could not but be, a permanent and most instructive lesson;—perpetually correcting lurking mistakes, and suggesting new thoughts. And if, while I have looked at your science in this spirit, you have thought me worthy to be called to preside over your body for two years; and if, during that time, you have not repented of your choice, as I have not found my views inapplicable to the subjects which have come before you; I may, I would believe, find in this some ground for confiding in the trains of thought which have thus led me to such a position; and may hope that, however arduous be the task of framing a philosophy of science suitable to its present condition, and of using such a philosophy as a means of furthering knowledge in general, still,

that in this task, to which our age is so manifestly called, I too may be a helper.

I trust that you will excuse these few words uttered with reference to my own peculiar pursuits, since these include yours also, and are my only claim to your indulgence. And now, Gentlemen, that I may trespass upon that indulgence no longer, I once more thank you in all earnestness and sincerity for your good opinion which placed me in this chair, and for the kindness and support which I have on all occasions received from you; and with my best wishes for your prosperity, and that of your science, I resign my office into abler hands.

Feb. 27.—A paper was first read, entitled "An Account of Impressions and Casts of Drops of Rain, discovered in the Quarries at Storeton Hill, Cheshire," by John Cunningham, Esq., F.G.S.

The author commences by stating, that no person acquainted with Geology, can doubt of rain having fallen during remote ages of the world, because to its destructive and transporting powers many of the sedimentary strata must have owed their origin. He also observes, that the vast forests which flourished anterior to the era of the new red sandstone, and are now treasured up in beds of coal, could not have existed without abundant supplies of atmospheric waters. Mr. Cunningham refers likewise to Mr. Scrope's account of the permanent preservation of the effects of a shower, which fell on extremely fine ashes, thrown out by Vesuvius during the eruption of 1822. The drops of rain formed globules which resembled in shape and motion those produced by sprinkling water on a dusty floor; and the globules afterwards hardened into pellets, which accumulated, at the bottom of a slope in some places, into beds a foot or more thick; and they afterwards became so firmly agglutinated, that it required a smart blow from a hammer to break the mass.

The effects of rain described by Mr. Cunningham, are, however, of a kind entirely different from those produced on the ashes of Vesuvius. They were discovered by him in the sandstone quarries in which the footsteps of the Chirotherium were found*; and he was the first to assign their origin to the effects of rain. The under surface of two strata, at the depth of 32 and 35 feet from the top of the quarry, present a remarkably blistered or warty appearance, being densely covered by minute hemispheres of the same substance as the sandstone. These projections are casts in relief of indentations in the upper surface of a thin subjacent bed of clay, and due, in the author's opinion, to drops of rain. On one of the layers of clay, they are small and circular, as if produced by a gentle shower; on the other, they are larger, deeper and less regular in form, indicating a more violent operation, possibly accompanied by hail. On the surface of these layers of clay there are also impressions of the feet of small animals, which appear to have passed over the clay either during the showers or not

* See the Memoir by the Committee of the Natural History Society of Liverpool, p. 12.

long before, as the footsteps are indented by the drops of rain, but to a less degree than the untrodden parts, in consequence, the author conceives, of the pressure which the clay had undergone beneath the feet of the animals. Ripple marks are exhibited also on the surface of many sandstone strata in the same quarries; and the rain marks as well as the sharpness of many of the footsteps prove, that the clay was not covered by water during the shower, or while traversed by the animals; and Mr. Cunningham, therefore, is of opinion that the conditions necessary to the preservation of such impressions, particularly of the rain drops, would be a return of water over surfaces which had been left uncovered during an interval too short for the desiccation of the laminæ of clay before the shower fell; and which were sufficiently soft to receive the impressions, as well as tenacious enough to retain them, until the return of the water which filled the prints with sand. Another condition is, that the velocity of the water charged with the sand was not sufficient to overcome the tenacity of the clay, or disturb the impressions of the rain drops. The author adds, that Dr. Buckland has suggested to him, that the interval between the rise and fall of tides over extensive sandbanks, the surface of which was between the level of high and low water, might have afforded daily occasions for the fulfilment of all the conditions; and that it is not easy to explain the alternate exposure to air and submersion under water without appealing to the flux and reflux of tides.

An extract was then read from a letter addressed to Dr. Buckland, by John Taylor, jun., Esq., F.G.S., on a slab of sandstone, exhibiting footmarks, and supposed to be from the Kelsall quarry, at the foot of Delamere Forest, but now in a pavement in the house of Mr. Potts, of Chester.

A letter was next read, addressed to Dr. Buckland by Sir Philip Grey Egerton, Bart., M.P., F.G.S., respecting the same slab; and accompanied by a tracing of the foot-marks, by Miss Potts.

When the slab was first laid down, there were no indications of the footsteps, and Sir Philip Egerton explains, in the following manner, their origin in a homogeneous stone and subsequent development. The weight of the animal on the soft sand compressed the yielding materials in the vicinity of the foot, and the print having been filled with sand, the stone, on becoming indurated, would present a nearly uniform texture. The action of the weather, on the flag being exposed, would remove the softer portions of the surface, and the denser parts surrounding the impressions of the feet, would resist the same operation, and present in relief the outline of the foot.

The flag contains the prints of three hind and two fore feet, the latter bearing nearly the same proportions to the former as in the other species, but Sir Philip Egerton could not make accurate measurements, because the markings are not all on one plane; the length of the stride he was also unable to determine, in consequence of the impressions in the same line being all of the right foot. There are distinct marks of claws on several of the toes.

A paper was next read, "On the occurrence of numerous Swallow

Holes, near Farnham; with some observations on the drainage of the country at the western extremity of the Hog's Back," by Henry Lawes Long, Esq., and communicated by C. Lyell, Esq., V.P.G.S.

Farnham stands at the foot of the chalk hills, upon a deep bed of loam, which appears to overlie the gault. Upon the chalk, immediately to the north of the town, is the castle, beyond which the tertiary strata commence and rise to a considerable height, forming the great mass of hill known by the name of Farnham Beacon, Tunbury, or the Lawday House. On the north side, this hill presents, for the greater part, an abrupt precipice, under which several streams are thrown out; but on the south there are landsprings only, which occupy the gullies for the greater portion of the year, and occasionally become formidable torrents. These rivulets pour down the tertiary clays until they arrive at the chalk, where they plunge into the ground and disappear, except during very heavy rains, when the surplus waters are carried off by gravely channels in the chalk.

The principal object of the paper is to describe the seven swallow holes between Clear Park and Farnham Park, and a minute account is given of each. They occur in Clear Park—Lower Old Park Gully—Clay-pit Gully—near the Potter's Clay-pit—in the Hop-grounds, above the turnpike a little west of the Odiham-road—near the entrance of the pleasure ground in Farnham Park—and near the end of the avenue at the east of Farnham Park. The water absorbed by the holes in Farnham Park is supposed to reappear at the Bourne-Mill-stream; and though soft where it sinks into the chalk, it is hard and unfit for use, where it again breaks forth. The existence of underground currents was further proved by a well sunk at Hale Farm, which gave the following section:

Sand and gravel	6 feet.
Clay (potters')	15 or 16.
Sand and gravel	20.
Clay (potters')	14 or 15.
Clay, blue (London?) lowest 2 feet a green sand	24.
Hard chalk	20 or 30.

At that depth a spring was reached, which was supposed to be the Bourne-Mill-stream, and the instrument went down rapidly many fathoms, through a chalk mud. The well-sinkers afterwards came upon chalk with many flints, and finally breaking their instrument, left 80 feet of it in the earth, having bored altogether to a depth of 176 feet.

The green-sand tract, described in the second part of the memoir, and drained by a stream which flows northward through a gap in the chalk at Runfold into the London basin, is bounded on the north by the straight line of the Hog's Back, and on the south by a semicircular range of the low hills extending from Seale on the east by Crooksbury Hill to Moor Park on the west. The surface of the tract being sandy and naturally bibulous, the proprietor of the farm has rendered it more retentive by a system of marling, and the rain water being consequently less absorbed than formerly, it is collected in an excavation called White-ways End Pond, at the western end of the

Hog's Back. From this pond a small stream flows towards Runfold, and passing thence across the depressed chalk, continues its course to the county stream, or Blackwater river, receiving apparently a small augmentation from a spring at Andrew's hop-kiln. This gap in the chalk at Runfold, not having been hitherto noticed by geologists, Mr. Long conceives, that it deserves to be recorded among the apertures of the North Downs.

An extract was last read from a letter addressed to Mr. Lyell by Capt. Charters, F.G.S., and dated Cape Town, Nov. 12, 1838.

During an extensive tour through the colony, Capt. Charters's attention was drawn to a vast deposit of greenstone, overlying the horizontally stratified sandstone which occupies so large a portion of Southern Africa. The following localities are mentioned in the letter. A hill close to Fort Beaufort, on the Kaffir frontier. The banks of the Great Fish River, near the small town of Cradock, in the neighbourhood of which quantities of spherical masses of trap are heaped together, the surrounding sandstone mountains being of considerable elevation, and having their flanks and sometimes their tops very frequently covered with loose fragments of trap. On the right bank of the river and about a mile from the town, is exhibited a section, consisting in the lowest part of inclined strata of clay slate, in the middle of horizontal beds of sandstone, and in the uppermost of masses of trap. The same geological structure prevails in passing through the Tanka district, behind the Winterberg range to Shiloh, and thence to Colesberg, near the Orange river. From Colesberg, Captain Charters proceeded to Graf Keynet by the Schneeberg, and he found that the only variation in the nature of the country consisted in a considerable diminution of the quantity of greenstone. The left of a narrow gorge through which the Sunday river passes, presents an abrupt precipice 300 feet high and as many yards long, composed of columnar greenstone resting at its foot on horizontal strata of sandstone.

March 13.—A paper on the geology of the North Western part of Asia Minor, from the peninsula of Cyzicus, on the coast of the sea of Marmara, to Koola, with a description of the Katakekaumene, by William John Hamilton, Esq., Sec. G.S., was read.

The memoir is divided into two parts, the first containing an account of the country between Cyzicus and Koola, the second a description of the Katakekaumene.

The line of route taken by Mr. Hamilton from Cyzicus, ascends the valley of the Macestus to the sources of that river near Simaul, then crosses the Demirji chain, and afterwards passes through Karskieui and Selendi to Koola, in the Katakekaumene, the whole distance being about 170 miles. The principal leading feature of the district is the Demirji chain reaching from Pergamum on the west, to the lofty mountain of the Ak Dàgh or Shapkhana Dàgh on the east, and it is prolonged in that direction by a lofty range which extends E.S.E. to Morad Dàgh, south of Kutàhiyah, and thence by Aiom Karahissar to Sultan Dàgh, an extension of one of the chains of Mount Taurus, so that the Demirji range forms a portion of the central axis of Asia

Minor. The country traversed by Mr. Hamilton is also intersected by numerous hills, some of which exceed 1200 feet in height. The lake of Maniyas is another marked feature in the district. The formations of which the country is composed, are,—1, schistose rocks with saccharine marble; 2, compact limestone resembling the scaglia of Italy and Greece; 3, tertiary sandstones; 4, tertiary limestones; 5, granite; 6, peperite; 7, trachyte; 8, basalt. Between Kespit and the Demirji chain is a deposit of white marl, which Mr. Hamilton is of opinion, was accumulated in an ancient lake drained by some of the igneous operations which dislocated the horizontal tertiary limestone, and formed the traverses in the high hills between Kespit and Susugerli.

1. The schists are composed of gneiss, mica slate, and clay slate, and they are associated with crystalline limestone. Argillaceous schists and marble occur between Cyzicus and Erdek; and thickly wooded hills, 1000 feet in height, which rise abruptly from the shore of the sea of Marmara, are capped by a fine marble. A little further eastward are extensive quarries of the same stone, to which Cyzicus was partly indebted for having been ranked among the most splendid cities of antiquity. The limestone is interstratified with indurated marls and shales of various colours; the whole dipping from 70° to 80° S.E. by S.: and near Erdek S.W., or in each instance from the granitic nucleus of Cyzicus. Similar schists occur in the Demirji range, and in the Katakekaumene, associated with limestone.

Between the 33rd and 34th miles from Simaul towards Koola, is a low ridge of hills of saccharine limestone, rising above the plateau of horizontal limestone, and belonging to the same formation as the hills about Koola. In the Katakekaumene, the older system of volcanic cones is situated on these schists, and the newer in the adjacent alluvial plains, an important distinction accounted for in the description of that district.

2. *Compact Limestone* resembling the scaglia of Italy and Greece occurs only south of the lake of Maniyas, and at the foot of the range of hills near the town of the same name. It is associated with beds of shale. A micaceous sandstone, which forms a range of broken and water-worn hills between Mülverkieui and the valley of the Susugerli or Macestus, is considered by Mr. Hamilton, to be perhaps of the age of this limestone, as well as the high and broken range of hills between Ildij and Kespit.

3. *Tertiary Sandstones.*—This formation is very extensively developed, and consists of micaceous sandstones, sands, marls, and shales. No organic remains were noticed in it by the author. It ranges southward from the village of Susugerli for about two miles. At the eastern extremity of the Demirji chain, where it was traversed by Mr. Hamilton, thinly laminated micaceous sandstone rests against the granitic nucleus, and extends thence to the South for nine miles. This formation is also exhibited about 16 miles from Simaul, underlying irregularly and conformably the peperite, and at the 18th mile the junction between the peperite and the sandstone is well ex-

hibited. The lower volcanic beds are contorted, and consist of large masses and boulders of primary, igneous, and scoriaceous rocks; the beds, however, gradually become finer in the ascending order, and nearly horizontal in their position. In the sandstone the author noticed no fragments of volcanic matter. At the 19th mile, however, there appears to be a gradual passage or interstratification between the upper beds of the sandstone and the lower beds of the peperite. The sandstone and peperite extend along the valley of the Selendichai, and the former constitutes the hills between the valleys of the Selendi and the Hermus, and is capped by the white limestone. The beds throughout the country are nearly horizontal, except where they have been disturbed by igneous rocks.

4. *Tertiary Limestone.* This deposit Mr. Hamilton considers as belonging to the great lacustrine formation which occupies so large a portion of Asia Minor, but within the range of country described in this paper, it appears to be destitute of organic remains. It presents table lands composed of beds of white, compact, or thinly laminated limestone resembling chalk, and sometimes containing nodules of opaque white flints, and sometimes extensive beds of tabular flint. Near Kespit it is chalky, as well as 8 miles further south. It forms the hill on which stands the castle of Bogaditza, at the south-eastern extremity of the plain of the same name. South of the Demirji chain, and about eleven miles from Simaul, a white limestone overlies peperite, and a few miles further, rests upon trachyte. About the 19th mile, trachytic conglomerate overlies horizontal beds of white marl irregularly associated with beds of quartz pebbles. Between the valleys of the Selendi and the Hermus white limestone rests upon the micaceous sandstone, the volcanic products having thinned out. About the 35th mile, in the bottom of a ravine, Mr. Hamilton noticed the following section:

Lowest part, gravel and loose beds of sand....	30 feet.
Alternations of marls and sands, the former predominating in the upper part	20 do.
White marl	5 to 6.

Mr. Hamilton believes that the last bed passes into the white limestone. The hill above the ravine is capped by basalt in some places 100 feet thick, but a stratum of sand is occasionally interspersed between the limestone and the basalt. South of the Hermus an insulated patch of limestone is also overlaid by basalt, and around its base are lava streams which have flowed from the volcanic cones near Koola. The lower part of this patch of limestone is converted into a yellow jasper-looking substance, with a bright conchoidal fracture.

5. *Granite* occurs near Cyzicus, where it is a finely grained, gray rock, which decomposes rapidly; but it contains large masses of hornblende, and is sometimes traversed by veins of felspar. It throws off the adjacent schistose rocks, which dip from it in opposite directions. Granite apparently forms also the axis of the Demirji range.

6. *Peperite.*—This deposit is extensively developed in many parts of Asia Minor. It is distinctly stratified, but it has sometimes a

crystalline or vitreous aspect, and contains crystals of hornblende as well as much glassy felspar. Within the range of Mr. Hamilton's route it occurs about $2\frac{1}{2}$ miles south of the village of Susugerli; also 9 miles south of Simaul: and a little further the author obtained the following descending section:

1. Hard volcanic tuff, slightly crystalline, but containing many boulders and pebbles of trap, with numerous concretions of green marl, 12 feet.
2. Soft whitish volcanic earthy tuff, containing small fragments of pumice, 10 feet.
3. Hard crystalline but stratified rock.

About the 11th and 12th miles from Simaul, peperite is overlaid by a white limestone; between the 15th and 16th it rests upon protruded masses of decomposing trap or syenite; and half a mile further a mass of trachytic or trap conglomerate, forming the point of separation of two valleys, has been raised up subsequent to its deposition by a protrusion of trap, as the conglomerate, which is much contorted, adheres to the side of the trap; and near the 16th mile, it is underlaid by the micaceous sandstone.

The beds are occasionally horizontal, but where the peperite has been affected by the trachyte, they are variously inclined.

7. *Trachyte and Trachytic Conglomerate.*—Several varieties of this rock occur within Mr. Hamilton's district. The points more particularly mentioned are, one mile south of Kespit, where it forms a ridge of hills; the village of Kalburja, 7 miles S.W. of Kespit; also near the town of Bogaditza, whence a high trachytic range extends for a considerable distance east and west, succeeded by a less elevated district of the same rock, which continues beyond Singerli to the foot of the Dimirji mountains. In this district the trachyte varies greatly in colour, is generally soft, decomposes easily, and the author was often unable to decide whether it was an aqueous deposit of volcanic sand, or a subaqueous igneous rock. To the east of Singerli is a large mass of red porphyritic trachyte, considered by Mr. Hamilton to be a coulée which has flowed from the high rugged hills to the south-east. The trachytic rocks continue up the valley of Macestus for several miles. It is also extensively developed south of the Demirji chain between Simaul and Koola, particularly about the 13th or 14th mile from the former, and is overlaid by white limestone. About the 19th mile, in some places, cliffs of trachytic conglomerate rest upon the peperite, and in others the trachytic conglomerate overlies horizontal beds of white marl belonging to the white limestone, and interstratified as before stated, with irregular beds of quartz pebbles.

8. *Basalt* is exposed south of the Demirji chain at several places, but more particularly near and in the Katakekaumene.

A spur of porphyritic trap occurs about two miles south of Susugerli.

Hot Springs burst forth in great force about $7\frac{1}{2}$ miles east of Singerli. Their temperature is supposed by Mr. Hamilton to be equal to that of boiling water. Extensive depositions, in one part

8 or 10 feet thick, occur around the mouths of the springs; and a strong sulphureous smell accompanies the emission of the water; but where the temperature had become sufficiently low to permit the water to be tasted, no peculiar flavour was perceived. After flowing a mile and a half and turning several mills, the water is used for a warm bath. The rock from which the springs rise, is a greenish brown porphyritic trap. Some copious hot springs issue near the lower beds of the tertiary white limestone, a little north of Koola, the temperature varying from 123° to 137° Fahr. Two of them are situated in the centre of the ruins of an unknown ancient city. Mr. Hamilton perceived a slight development of sulphuretted hydrogen gas.

The Katakekaumene.—The extent of this interesting tract is much less than is assigned to it in published maps, being not more than 7 miles from north to south, and 18 or 19 from east to west. After alluding to his first visit to it in company with Mr. H. E. Strickland, and referring to that gentleman's account of a portion of the district*, Mr. Hamilton describes minutely the two systems of volcanos, distinguished by the state of preservation of the craters and of the coulées; he defines also the course of each lava-current, and points out its attendant phenomena—but these details admit of only partial abridgement.

The volcanic products are basalt, lava, and ashes, the first being confined to the more ancient craters, and the last to the more modern. The numerous older cones are further distinguished by being situated on parallel ridges of gneiss and mica slate, and the newer, only three in number, by being confined to the intervening alluvial valleys. This important distinction Mr. Hamilton explains on the supposition, that the elevation of the schistose ridges produced cracks, through which, as points of least resistance, the first eruptions of lava found vent; and that these openings becoming subsequently plugged up, by the cooling of injected molten matter, the schists were rendered so solid, that when the volcanic forces again became active, the lines of least resistance were transferred to the valleys.

The coulées from the ancient craters appear to have been partly under water, as their surface is, in some places, covered with sediment and turf; but the lava streams from the modern are bare, rugged, and barren, and the craters are surrounded by mounds of loose scoriæ and ashes. In addition to the comparative view given by Mr. Strickland of the phenomena of the Katakekaumene and Central France, Mr. Hamilton enters into a more extended investigation of points of resemblance, including other portions of Asia Minor. The great volcanic groups of Mont Dore, the Cantal, and Mont Mezen, Mr. Hamilton conceives are represented by Ak Dàgh, Morad Dàgh, the trachytic hills east of Takmak, Hassan Dàgh, and Mount Argæus. The modern volcanic period of Central France he compares with the Katakekaumene, as respects the composition of the lavas, their arrangement at different levels, and the cones being

* See L. & E. Phil. Mag. vol. x., p. 70.

scattered, not collected in great mountain masses. The Katakekaumene, in Mr. Hamilton's opinion, exhibits also additional evidence, that the disposition of comparatively recent volcanos is coincident with the strike of the granitic axis, from the interior of which the volcanos have burst forth. The author also alluded to other comparative phenomena noticed in Mr. Strickland's paper. Lastly, he pointed out two distinctions:—in Central France streams of igneous products may be traced from the most ancient volcanic masses of Mont Dore, but in Asia Minor none have been detected which could have flowed from Ak Dàgh, or Morad Dàgh. In France, also, trachytic eruptions occurred during the deposition of the lacustrine limestone; but in the Katakekaumene, they appear to have preceded that of the white limestone, or are associated with only its lowest beds.

In conclusion, the paper gives a general summary of the geological phenomena of the country south of the Demirji range.

The relative antiquity of the vast lake or sea in which the strata were deposited, cannot be determined, as the micaceous sandstone forming the lowest series of beds is apparently destitute of organic remains, and Mr. Hamilton, therefore, does not attempt to compare that deposit with any European formation. The sandstone, he conceives, was accumulated upon an irregular surface of schistose rocks and crystalline limestone, and before the elevation of the Demirji chain. Upon the sandstone were deposited in the north of the district the beds of peperite, derived probably from subaqueous volcanos; and upon the peperite and the micaceous sandstone, the white limestone, which is the highest sedimentary rock. The drainage of the lake, he is of opinion, took place during the earliest volcanic eruptions of the Katakekaumene.

Three well-defined periods of igneous operations may be traced. The first is marked by the masses of basalt which cap some of the plateaux of white limestone, and were ejected previously to the country assuming its present configuration, and to the formation of the valleys. Mr. Hamilton considers that the basalt flowed under water, and probably but a short time before the drainage of the lake.

The second period is characterized by the currents of basalt and lava from the ancient system of volcanos in the Katakekaumene, and was subsequent to the formation of the present valleys, as many of the lava streams may be traced into them. The coulées which flowed towards the Hermus from the crater or Karadevit near Koola, present an inclined plane, the surface of which is not more than 150 or 200 feet above the present bed of the river; but they must, at one period, have been under water, as the lava is covered with a sediment which fills its crevices and smooths its asperities.

The third period belongs to the more modern system of cones, the lava of which is as rugged and barren as the recent coulées of Etna and Vesuvius. Of the date of these eruptions, Mr. Hamilton offers no opinion, merely remarking that the craters are mentioned by Strabo, and that there is no tradition of their activity.

March 27.—A paper was read by Prof. Owen, F.G.S., entitled a

"Description of a Tooth and part of the Skeleton of the Glyptodon, a large quadruped of the Edentate order, to which belongs the tessellated bony armour figured by Mr. Clift in his memoir on the remains of the Megatherium, brought to England by Sir Woodbine Parish, F.G.S."

The first notice of the remains of a fossil large edentate Mammal associated with a tessellated bony armour, is an extract from a letter addressed by Don Damario Larrañaga, Curé of Monte Video, to M. Auguste St. Hilaire, and appended to Cuvier's account of the Megatherium in the Ossemens Fossiles, t. v. p. 179. (1823). The bones were discovered near the surface in alluvium, in the Rio del Sauce, a branch of the Saulis grande, and consisted of a femur 6 to 8 inches in width, but short, and in every respect like the femur of an Armadillo; also a portion of a tessellated bony armour. The tail is described as very short and very stout, and to have had a bony armour, which was not verticellate or disposed in rings. Similar fossils are said to occur in analogous strata near the lake Mirine, on the frontier of the Portuguese colonies. The notion that the remains found in the Rio del Sauce belonged to the Megatherium, rests solely on the circumstance of Don Damario Larrañaga having inserted the word Megatherium as the synonym of his gigantic fossil "Dasypus." Je ne vous écris point sur mon Dasypus, (*Megatherium*, Cuvier.)

The next observations bearing upon the present subject are contained in Weiss's Geological Memoir on the provinces of San Pedro do Sul, and the Banda Oriental, (Berlin Trans., 1827). These remains consisted of part of a femur of a Megatherium, without any associated armour, found at a deserted Indian camp near the Queguay, a tributary of the Uruguay; of portions of osseous tessellated armour, apparently unaccompanied by bones, discovered on the Arapey chico, in the province of Monte Video; and of bones of the extremities and fragments of armour found near the Rio Janeiro. The whole of these remains were collected by Sellow, the Prussian traveller, and after his death the last-named collection of bones and armour were submitted to Prof. D'Alton, by whom they have been described, (Berlin Trans., 1833,) and who states, that they are not the remains of the Megatherium, but of a large edentate animal more nearly allied to Dasypus.

In 1832, Mr. Clift laid before the Geological Society a memoir on the remains of the Megatherium brought to England from Buenos Ayres by Sir Woodbine Parish*. In the collection of which they formed a part, were fragments of bony tessellated armour, one of which was figured but not described by Mr. Clift, because the fragments were not associated with the remains of the Megatherium; there were also a portion of a jaw and several other bones, which were found in connexion with portions of a bony armour in the bed of a rivulet at Villanueva, about 95 miles south of Buenos Ayres. On the examination of the last-mentioned remains when they first arrived in England, it was evident both to Mr. Clift and Mr. Owen, particularly from the conformation of the alveoli in the jaw, that the bones did

* See L. & E. Phil. Mag. vol. i. p. 233.

not belong to the Megatherium; and that the dentition of the extinct species differed more widely from that of the existing subgenera of Armadillos than the respective dental characters of the latter differ from each other. As the portions of the skeleton were not sufficient to enable Mr. Clift to determine satisfactorily the characters of the animal, no account of them was given in his memoir on the Megatherium, but they form the subject of Mr. Owen's paper, of which this is a notice. Soon after the arrival of Sir Woodbine Parish's collection, the College of Surgeons had casts made of the bones, and presented them to different museums, including the Jardin du Roi, where they were examined by M. Laurillard and Mr. Pentland. These naturalists also concluded, especially from the bones of the foot, that the remains were not portions of the Megatherium, but of a gigantic Armadillo.

More recently, Sir Woodbine Parish received an account of the discovery, in the bank of a rivulet near the Rio Matanza, 20 miles south of the city of Buenos Ayres, of a perfect skeleton and bony covering, and with the description, he also received a fragment of a tooth and a drawing of the animal. On examining the tooth, Mr. Owen found, that it belonged to an animal referable to the Edentata of Cuvier, but indicative of a new sub-genus of the Armadillo family; and for which he proposed the name of *Glyptodon*, in reference to the sculptured character of the tooth. Subsequently, he compared the tooth with the alveoli in the fragment of the jaw in Sir Woodbine Parish's collection; and he found that the peculiar longitudinal ridges in the sockets precisely corresponded with the flutings in the tooth itself, whereby he was enabled to prove, that the bones discovered with the tessellated coat of mail at Villanueva appertained to the same species as the more perfect skeleton and cuirass found near the Rio Matanza.

Judging from the drawing transmitted to Sir W. Parish, the Glyptodon differs from the Megatherium not only in the form and structure of the teeth, but in the number, which appears to be eight on each side of each jaw; and from all known Armadillos in the form of the lower jaw, as well as in the presence of a long process descending from the zygoma, in both which respects it resembles the Megatherium. According to the same figure, the tail was protected by a narrow bony covering on the upper surface only, and was not encompassed by it as in the Armadillos.

Mr. Owen then proceeds to describe the remains of the Glyptodon which have arrived in England. The molar tooth is only a fragment, but the grinding surface and upwards of an inch of the crown are perfect, the whole length being about two inches. There is no indication of a diminution in any of its diameters from the grinding surface to the opposite end, and the alveoli in the fragment of the jaw terminate abruptly without any contraction. The teeth are more compressed than those of the Megatherium, and differ from them in intimate structure, resembling in this respect the teeth of the Armadillos. From all known Armadillos, the Glyptodon, however, is distinguished by the tooth having on both the outer and inner

surfaces two deep grooves, each extending from the opposite sides about one third of the transverse diameter of the tooth and through its whole length, dividing the grinding surface into three portions, joined together by the contracted isthmus interposed between the opposite grooves. The teeth thus exhibit a more complicated form than those of any known Edentate, and seem to indicate a transition from that family to the Pachydermal Toxodon.

The fragment of the jaw discovered at Villanueva consists of a portion near the extremity of the left ramus, and includes three alveoli, which slightly increase in size as they are placed further back.

The humerus, of which the distal half has been received, agrees most nearly with that portion of the humerus of the Dasypus, but the internal condyle is not perforated; the depressions also above the trochlea, both in front and behind, are relatively deeper, and in the side opposite the deltoid trochanter there is a rugged raised surface for a muscular insertion, of which Mr. Owen has not perceived anything analogous in the Armadillos. From the humerus of the Megatherium it differs in not presenting the extraordinary expansion of the distal extremity exhibited in that animal; but the internal condyle in the Megatherium is also imperforate.

The radius of the Glyptodon corresponds very nearly with that of the Armadillo, but it differs from the radius of the Megathere in being three times less in every dimension, and by well-marked differences in all the details of structure.

The ungueal phalanges of the Glyptodon approach most nearly those of the species of Dasypus; but in their shortness, as compared with their breadth and depth, they resemble still more the ungueal phalanges of the Pachyderms. Mr. Owen is of opinion that they were encased in strong, short, hoof-like claws; and that they exhibit rather the base of an anterior column of support to an animal clad in a ponderous cuirass than instruments especially designed for scratching or digging. There cannot be a greater contrast than is presented between the short, broad, and flat phalange of the Glyptodon, and the long and compressed claw-bone of the Megatherium.

Of the posterior extremity of the Glyptodon, the tibia, which is anchylosed to the fibula, presents the structure characteristic of the tibia of the Armadillos; while in the Megathere the corresponding bones deviate widely in their proportions, and in the conformation of the distal articular surface from those of the Glyptodon. The conformation of the astragalus, calcaneum, the cuboid, scaphoid, and internal cuneiform bones, also of the metatarsals of the three middle and largest toes, the three phalanges of the second and middle, and the distal phalanges of the third and fourth toes, were described in great minuteness, but it is not possible to abridge the details.

Mr. Owen, however, stated that when the bones of the hinder extremity are arranged in their natural juxta-position, they present a foot of such singular proportions as to be without a parallel in the animal kingdom. The nearest approach to its broad, thick, short, and massive proportions is made by the skeleton of the fossorial extremity

of the Mole; but it is the fore foot only of this animal that can be compared in the compressed figure of the metacarpals and proximal and middle phalanges with the singular hind-foot of the Glyptodon. The hind foot of the Mole resembles in the lengthened metatarsal and phalangeal bones that of the existing Armadillos, and the generality of quadrupeds. The true structure of the hind foot of the Megatherium is not known, but in the terminal phalanges it differs most widely from those of the Glyptodon. In the former, the compressed lengthened shape is as extreme in the claw-bones as, in the latter, is the depressed, shortened figure. In the Glyptodon, the hind foot, like the fore, appears to be expressly modified to form a base to a column destined to support an enormous superincumbent weight; while in the Megatherium the toes were free to be developed into long and compressed claws, such as form the compensating weapons of defence of the hair-clad Sloths and Ant-eaters. The ungueal phalanges of the Armadillos, in their shorter, broader, and flatter form, make a much nearer approach to those of the Glyptodon; and it may be readily admitted that the hind foot of the Glyptodon is an extreme modification of the same general plan of structure as that on which the foot of the Armadillo is constructed; but if the differences in the tarsal bones (described in the paper) exceed those which are traceable between one species of Armadillo and another, *a fortiori*, the antero-posterior compression of the metatarsals and phalanges, and the total suppression in those of the ginglymoid trochlear articulations are indicative of a difference of general habits, as great as is usually observed in animals of distinct but nearly-allied genera. Thus both the dental modifications and the locomotive organs prove that the Glyptodon cannot be called an Armadillo without making use of an exaggerated expression; still less can it be considered a species of Megatherium; but it offers the type of a distinct genus, which is much more nearly allied to the Dasypodoid than to the Megatherioid families of Edentata. For this genus Mr. Owen had proposed a name indicative of its dental peculiarities, and, as the present species agreed with the Armadillos in its dermal armour, he preferred the name of *Glyptodon clavipes*, in relation to the peculiar modification of the foot.

Mr. Owen then showed that the portions of tessellated armour described and figured by Weiss are identical in structure with those brought to England by Sir Woodbine Parish, and that the bones which were found with the armour in both cases belonged to animals specifically identical. He next entered upon the inquiry, Had the Megatherium a bony armour? and he concluded from a comparison of its skeleton with that of the Armadillos, that it had not. In the pelvis of the Armadillo there are twelve sacral vertebræ anchylosed together, and the spines of the vertebræ are greatly developed antero-posteriorly, forming a continuous vertical ridge of bone, bearing immediately the superincumbent weight. In the Megathere the sacral vertebræ are only four in number, and are not anchylosed, and the spinous processes are comparatively small, not locked together, as in the Armadillos, but separated by intervals as in the Sloths. In the

Armadillos, the weight of the cuirass is transferred from the sacrum to the thigh-bones by two points on each side. One of them, the ischium, is anchylosed to the posterior part of the sacrum, the other point is formed by the conversion of the iliac bone into a stout three-sided beam passing straight from the thigh-joint to abut against the anterior part of the sacrum, where the weight of the shell is greatest,—a structure which is wanting in the Megathere. In no species of Armadillo is the ilium expanded, while in the Megathere it is greatly developed, resembling that of the Elephant in size, form, and position ; and among the Edentata the nearest approach in this portion of the skeleton is to be found among the Sloths and Ant-eaters. The most striking point however, in the structure of the Armadillos, with reference to the support of a bony covering, is the remarkable production of a part of the vertebra from above the anterior articular process on each side, in a straight direction upwards, outwards, and forwards, to nearly the height of the true spinous processes. Now, these oblique processes, which are developed only in the loricated Edentata, beautifully correspond in form and use with the tie-bearers in the architecture of a roof, and are entirely wanting in the Megathere, the structure of this part of the vertebral column of that animal corresponding with the character of the vertebræ of the hair-clad Sloths and Ant-eaters. Mr. Owen noticed other supposed adaptations in the skeleton of the Megathere to sustain a bony covering, as the breadth of the ribs, but the ribs of the Sloths and Ant-eaters are broader than those of the Armadillos.

The paper contained a tabular account of the discovery of twelve skeletons of the Megathere, and in no instance did any portion of bony armour occur with or near the bones. A notice was also given of the remains of a Glyptodon, found in the left bank of the Pedernal before its junction with the Sala, an affluent of the Rio Sante, near Monte Video, and preserved in the museum of that town. From the accounts which have been given of these remains they appear to have belonged to the same species as that described in the paper. An allusion was also made to some portions of bony armour obtained in the Rio Seco, in the Banda Oriental, and similar in structure to the specimen of the Pedernal. One of the portions was the covering for the tail. It was hollow to its extremity, and presented in its concavity, vestiges of caudal vertebræ very distant from each other.

In conclusion, Mr. Owen observes, that having brought together evidence of the remains of five specimens (found in the Rio Seco, Rio Janeiro, Villanueva, Pedernal, and the Banda Oriental) of a large Edentate species undoubtedly covered with armour, and more or less corresponding with the characters of the Glyptodon, and having established the characters of that genus on both dentary and locomotive organs ; he trusts that he has at the same time vindicated the opinion of Cuvier with reference to the Megathere, by proving it to be, by its tegumentary covering as well as its osseous system, more nearly allied to the Ant-eaters and Sloths than to the Armadillos.

ASTRONOMICAL SOCIETY.

March 8.—The following communications were read:—

Observed Transits of the Moon, and Moon-culminating Stars, over the Meridian of Edinburgh Observatory, from June 1 to December 31, 1838. By Professor Henderson.

Lunar Occultations of Planets and Fixed Stars, and Eclipses of Jupiter's Satellites, observed at Edinburgh Observatory in 1838. By Professor Henderson.

Moon-culminating Stars observed at the Cambridge Observatory in the Months of November and December, 1838. By Professor Challis.

Occultations observed at Dulwich and Ashurst, from July 31 to December 27, 1838. By Robert Snow, Esq.

On the Method of determining the Longitude by Moon-culminating Stars. By Mr. Epps, late Assistant-Secretary to the Society.

The author remarks, that the advantages of moon-culminating observations, for the purpose of determining the difference of longitudes, particularly in the case of distant meridians, are now universally admitted, every other method being found subject not only to greater trouble and difficulty, but to errors far exceeding the limits of those to which the results of moon-culminating observations are liable. But although this method, the merit of introducing which belongs chiefly to Mr. Baily, is justly regarded as the best known, yet the result of a single observation, or of a few observations, is liable to a considerable amount of error, even when the observations are made by the best instruments and the ablest observers; and the object of the present communication is to show the extent to which the error may be expected to reach.

The moon's proper motion, on which the method entirely depends, is such, that an error in the observation is necessarily greatly augmented in the resulting longitude; in fact, the error in longitude will always be from 21 times to 35 times the amount of the error of observation. If, then, it be assumed that $0^{s}\cdot 2$ of time is the probable error of an observed transit, and that the observed interval between the transits of the moon and star is liable to an error of $0^{s}\cdot 4$, the resulting difference of longitude will be in error from 8 to 14 seconds of time. This is the probable effect, from observations made at one station only; and as it is quite possible that the effect may be doubled by an equal amount of error, in a contrary direction, at the other station, the author thinks he will be within limits in assuming 10 or 12 seconds of time as the probable error of longitude, resulting from the comparison of a corresponding interval observed at two stations, admitting that the best means of making the observation exist at each.

In order to show that the probable error is not overrated, by assuming it to amount to 10 or 12 seconds, the author refers to two lists of results given by Professor Henderson, in the introduction to the first volume of the *Edinburgh Astronomical Observations*. One

of these lists contains the computed differences of longitude between Edinburgh and Greenwich, and the other between Edinburgh and Cambridge; each contains 36 combined results, some of which differ by a much greater quantity than the probable error above assumed. The mean of all the results, as deduced by Professor Henderson, likewise shows the degree of approximation which the method may be considered as capable of giving, in the most favourable cases. Each of the lists referred to consists of a *mean* of the differences of the observed intervals at the two places on the several days stated, so that each series of results contains about 100 corresponding intervals; and, consequently, the final difference of longitude, deduced from each list, is the mean result of 36 mean quantities, deduced from about 100 corresponding intervals, observed at both stations. In fact, there are 136 corresponding transits made at Edinburgh and Greenwich, and 133 at Edinburgh and Cambridge; and from all these the difference of longitude between Greenwich and Cambridge comes out $= 13^{m}\ 5^{s}\cdot 5 - 12^{m}\ 44^{s}\cdot 4$, or $21^{s}\cdot 1$. But the difference of the longitude of the two places, ascertained by other means, and, doubtless, with scrupulous exactitude, is $23^{s}\cdot 54$; so that the final result of the moon-culminating observations is wide of the truth by more than 2 seconds of time.

The author concludes by observing, that the results here referred to, instead of tending to depreciate the method, may be considered, on the contrary, as affording proof of its excellence; for, in respect of distant meridians, an error of 2 seconds is undeserving of notice. It is however evident, that, considering the inferiority of instruments, and other sources of error incident to a temporary station, a tolerable degree of accuracy can only be expected from an extensive series of corresponding observations.

On the Position of Lacaille's Stations at the Cape of Good Hope. By Thomas Maclear, Esq. M.A. F.R.S., Astronomer Royal at the Cape. Communicated by Captain Beaufort, R.N. F.R.S. This paper was in part read.

April 12.—The reading of Mr. Maclear's paper, on the Position of Lacaille's Stations at the Cape, was resumed, and concluded.

The astronomical celebrity of the Abbé de Lacaille's visit to the Cape of Good Hope in the last century, together with the remarkable results deduced from his arc of the meridian, naturally prompted Mr. Maclear to become acquainted with his stations, and to connect the southern, which was his observatory, with the present Royal Observatory. In undertaking this task, he soon found that the lapse of eighty-five years had obliterated all local evidence of the French astronomer's operations; and the fact that he was there at all was only kept alive by the inquiries of Captain Everest in 1821. Having carefully perused the various Memoirs of Lacaille relative to his Cape operations, as well as his printed Journal, Mr. Maclear applied to his Excellency Sir Benjamin D'Urban, the governor of the colony, for leave to inspect the official documents. This was readily granted; but although several letters and notices interesting to the astronomer were brought to light, nothing was discovered tending to promote

the object he had immediately in view, which was to identify the spot on which Lacaille's Observatory had stood.

Lacaille states, that he resided in the house of a person of the name of Bestbier, on whose premises the Observatory was built. Accordingly, Mr. Maclear undertook to trace the residences and property of all persons of the name of Bestbier; and he ascertained, on searching the records of the Transfer Office, that only one person of that name held property in Cape Town in the year 1751, and he answered to the description given by Lacaille. By tracing down the successive transfers of this property to the present time, Mr. Maclear was led to the house in Strand Street, now occupied by Mrs. De Witt; the house referred to by Captain Everest. This lady permitted him to inspect her title-deeds and diagram, and they were found to agree exactly with the records in the Transfer Office. The position and form of the premises also corresponded with several remarks made by Lacaille in describing his operations.

Having thus obtained undeniable proof of the identity of Bestbier's house with that now occupied by Mrs. De Witt, the search for the position of the Observatory was brought within narrow limits, for Lacaille states that it was in the court of the house where he lived. The author accordingly proceeded to take measures for connecting the house with the Royal Observatory by triangulation, resolving to spare no pains in the execution of this part of his operations, inasmuch as he entertained a hope that he should thereby be able to ascertain whether Lacaille's plumb-line had been affected by the attraction of Table Mountain.

In the meantime, Mr. Maclear had communicated his views and proceedings to Captain Beaufort, and Mr. Airy, the Astronomer Royal, the latter of whom immediately wrote to the Secretary of the Admiralty, requesting their Lordships' permission to send out Bradley's zenith-sector, in order that Mr. Maclear might be enabled to verify at once the amplitude of Lacaille's arc. This was precisely what he himself had wished to accomplish, provided he could identify the northern station.

The northern station, at Klyp-Fonteyn, cannot be so readily traced as that in Cape-Town. On visiting the place, accompanied by Lieut. Williams of the Royal Engineers, Mr. Maclear found that a close investigation into the history of the proprietors, in connexion with the buildings and ruins, would be necessary; for, on looking at the old foundation described by Captain Everest, as the platform of the granary in which Lacaille had observed, and comparing its dimensions with Lacaille's statement, and its position and distance from the old house, so unlike the usual arrangements of the Dutch farmers, he perceived strong reasons for doubting its identity with the granary of Lacaille.

The platform alluded to by Captain Everest consists of a foundation-wall 63 feet long, by 24 in breadth; a considerable portion of it, on the west side, is two feet above the ground, and it is situated at the distance of 630 feet from the old house. But Lacaille states that the place in which himself, Mr. Bestbier, and his

assistant slept, was a part of the granary, 6 feet long, and 7 feet wide, separated from that in which the sector was by a curtain, which formed a sort of partition; and beyond was a little place for the slaves. Now if this platform was the site of the granary inhabited by Lacaille, it is very difficult to account for these confined dimensions. It can hardly be supposed that the place was filled with grain, for Lacaille's visit took place about a month before the time of harvest. Besides, it was distinctly asserted by Ferrit Cotsee, an aged inhabitant of the place, that the foundation in question was the ruins of his father's dwelling-house. It therefore became necessary, in order to arrive at the facts, to inquire into the evidence on which Captain Everest had fixed on the spot; to investigate the truth of Ferrit Cotsee's statement; and, lastly, to examine the place, by turning up the soil. As none of these plans could be carried into execution immediately, Mr. Maclear, in the meantime, returned to the Observatory.

Captain Everest's statement, which is given in the first volume of the *Memoirs*, p. 261, is as follows: "In reference to this matter, it may not be amiss to mention, that the daughter of the quondam proprietor of Klyp-Fonteyn, now an aged lady, named Letchie Schalkevyk, is still in existence, and not only gives a narrative perfectly agreeing, but has pointed out the very platform on which the granary once stood." On applying to Mr. Hertzog, the assistant surveyor-general at the Cape, who had accompanied Captain Everest to Klyp-Fonteyn, for information respecting the particulars of their journey, Mr. Maclear ascertained that the aforesaid lady, who appears to have resided at some distance, was not brought to the place herself, but sent her son, who pointed out the site, according to directions from his mother. The son himself could have no local knowledge, for no one of the name of Schalkevyk had resided at Klyp-Fonteyn within the memory of the oldest inhabitant living in 1838; it may therefore be conceived that he would be likely to point to the only ruin visible, as the site which his mother had described to him from memory. This evidence is obviously not to be put in comparison with that of Ferrit-Cotsee, who had constantly resided at the place during sixty-eight years, and remembered having lived in the house over the foundation in question.

When preparing for his second journey to Klyp-Fonteyn, with Bradley's sector, Mr. Maclear applied to his Excellency General Napier, and obtained a corporal of sappers and private from the artillery corps, under his former companion, Lieut. Williams. Arrived at Klyp-Fonteyn, he found the widow of the brother of Ferrit Cotsee, who had been married at the age of seventeen, forty-eight years ago, and had known Klyp-Fonteyn ever since. At her marriage, she lived with her mother-in-law, who died at the age of eighty, and has been dead thirteen years. She had often heard her mother-in-law speak of the French astronomer's visit. When made acquainted with Mr. Maclear's object, she took him to a spot where was a ruin in her early years, stated by her mother-in-law to be the ruins of the house of Oker Schalkevyk, the father of the lady referred

to by Captain Everest. Nothing in the shape of a ruin was to be seen; but the sappers were set to work, and, in a couple of days, exposed the foundations of a building, 54 feet long by 12, with three partition walls, and an oven at one end: evidently the site of a dwelling-house, as had been described.

While this operation was going forward, Ferrit Cotsee pointed out another spot, on which, he said, there had formerly been an old ruin. The sappers were again set to work; and, at the depth of about 3 feet, encountered a wall, which they traced and cleared, with great labour, in three or four days. This foundation, like the other, is of stone and clay. It is 22 feet by 12, and generally from 2 to 3 feet below the surface. Mr. Maclear attributes the depth to the sliding down of the soil from above, the ground over it being much inclined. Portions of chaff or short straw were found deep in the clay; but little confidence could be placed in this, as affording indications of a granary; for they might have been carried down by ants or mice. Ferrit Cotsee could give no account of the purpose of this building; there had been no roofed building there within his recollection.

There were now three ruins exposed to view, and the question arose, whether any one of these was the granary of Lacaille? The first was asserted by Ferrit Cotsee to be the ruins of his father's dwelling-house; the second was discovered exactly under the spot which his sister-in-law, from his mother's information, had described as the site of Oker Schalkevyk's house; with regard to the third, there was no direct evidence, but it stood relatively to the others in the position in which granaries usually stand in the country, and it was difficult to conceive any other purpose to which it could have been applied. Its dimensions were too contracted for a dwelling-house, and its masonry too good for carrying the flimsy hut of a hottentot or slave; which, besides, are not oblong, but circular, and never of more substantial materials than mud, except at the missionary institutions. After a minute and careful comparison of all the circumstances, Mr. Maclear came to the conclusion that the platform of Captain Everest was the dwelling-house of Cotsee's father, as the son asserts it to have been, and that the granary was on the foundation he had last exposed. This conclusion was subsequently confirmed by an entry discovered among the colonial records, from which it appeared that the proprietor of the place, in 1752, was Cornelius Cotsee, the grandfather of Ferrit; and that Oker Schalkevyk, supposed by Captain Everest to be the proprietor, was not a proprietor, but a householder, at the will of Cotsee. The meridional distance of the granary from the platform of Captain Everest is 210 feet, or rather more than 2″.

The signals at the other two angles of the triangle, namely, Riebeck's Castle and Capoc Berg, were easily recognised. The charcoal remnant of the signal-fire still remains on Riebeck's Castle, and was carefully covered up, by Mr. Maclear, with stones. The top of this rugged mountain, he observes, presents nothing inviting, and the ascent is laborious and difficult: hence the reason of the signal

remaining undisturbed; whereby the party were enabled to enjoy the sight of one undeniable mark of the work of Lacaille.

The author next proceeds to describe the operations for connecting Lacaille's southern station with the Royal Observatory. With these he included another position, " one which," he remarks, " must ever excite the feelings and enthusiasm of the admirers of genius, moral worth, and almost unlimited talent—namely, the scene of Sir John Herschel's recent labours." This position being invisible from the Observatory, and also from Cape Town, it became necessary to choose a fourth station; and, accordingly, King's Blockhouse Battery was fixed upon, which commanded a view of the other stations, and also of the base line.

The site of the base is on the sandy plane to the north of the Observatory. The east end is defined by the centre of the meridian pillar of the transit room; and the west, by a gun, let into a large flag-stone, which was sunk in the ground to the depth of seven feet, and firmly fixed by ramming down the soil. The line is so little elevated above the sea, that a large portion of it is covered by water at spring-tides.

Almost the whole of the apparatus for the measurement of the base was constructed at the Observatory; and although the screws, brass-work, &c. were homely in appearance, they sufficiently answered the purpose. The twenty-feet measuring-rods, of which three were used, were of well-seasoned white deal, on the model of those employed by General Roy, on Hounslow Heath, excepting in a few minor details. The square brass ferule covering each extremity, was perforated with a hole three-eighths of an inch in diameter; and a brass screw, one-fourth of an inch in diameter, and nearly two inches long, was passed into the wood through the hole, without touching the ferule, and ground down perfectly flat. The head of this screw carried the division which defined the length of the rod; and it is evident from the arrangement, that it could only be affected by the expansion of the wood. Eight trestles were constructed on the model of General Roy's, having wood screws and moveable teak-wood tables; and the usual equipment of boring-rods and piquets prepared.

A contrivance suggested by Sir John Herschel for measuring the space between the divisions on the contiguous ends of the measuring-rods, when laid in line, was adopted, and found to answer well in practice; and which obviated the possibility of any mistake in registering. This consisted in rendering the space a constant quantity, which was effected in the following manner. A couple of crosses being drawn with a fine point on a slip of mica, about 2·3 inches from each other, and the divided surface turned downwards to prevent parallax, the crosses were seen through the transparent mica, like spider lines; and as they were placed, when used, over a fine line on brass, a neat bisection of the cross was easily obtained, with the assistance of a common magnifier.

The twenty-feet measuring-rods were compared with the four-feet

brass scale belonging to the Observatory. This scale is by Dollond. It is a thin brass bar, inclosed in a mahogany case, lined with baize, and was brought to England by Sir John Herschel, on his return from the Cape, for the purpose of comparison with the Royal Astronomical Society's standard*. On making the proper reductions for temperature, the standard measuring-rod was found to exceed 20 feet of the scale by about 1-50th of an inch.

The measurement of the base line was commenced on the 17th of June, Sir J. Herschel, Lieut. Williams, and Mr. C. Piazzi Smyth, taking part in the work; but it had only proceeded a short distance, when it was interrupted by an accident. While Mr. Maclear was adjusting the 66th rod, a sudden gust of wind blew it and the next off the trestles, whereby one of them was entirely destroyed, and the other injured. This accident compelled him to postpone the measurement until November, for before a new rod could be got ready the winter had set in, and the floods spoiled the line. The author remarks, that although the contrivances adopted for preventing similar accidents proved effectual, rods of this description are unfitted for a windy country like the Cape, being liable to short vibrations, which no clamping can control.

The measurement was recommenced from the meridian pillar on the 8th of November, and proceeded without further interruption. In the preceding June a pig of lead, weighing 56 lbs., was sunk in the ground, at the end of the 27th rod, and a cross on its surface adjusted to the plummet. On reaching the 27th rod, in the second operation, the lead was uncovered, and the plummet fell short of the cross upon it by nearly half an inch. As the place was frequently covered with water during the winter, it is probable the lead had shifted its position.

The measurement was completed in four days. On each day the rods were compared with the standard, and the proper reductions made to obtain the length in terms of the standard rod, which also was corrected to the temperature of 70°, this being the temperature engraved on the brass scale. The final results gave the distance between the centre of the meridian pillar and the centre of the gun $=2919{\cdot}364$ feet.

The angles were measured in the latter part of 1836, while the apparatus for measuring the base was in progress. The instrument employed for the purpose was the repeating instrument by Dollond, described in the first volume of the *Memoirs*. The signals on the Block-house Battery, the Observatory, and Base-line stations, were tripods, surmounted by hoops, and covered with white cloth. At Mrs. De Witt's house the signal was a circular disc, painted on the east chimney.

On computing the triangles, and making the proper reductions, the distance between Mrs. De Witt's chimney and the transit instrument was found to be 17,096 feet, and its distance from the per-

* The comparison has since been made by Sir John Herschel and Mr. Baily. The mean of sixty-three comparisons gave its length $= 47{\cdot}997083$ standard inches of the Society's standard yard.

pendicular to the meridian 4579·5 feet. Assuming the compression $= \frac{1}{310}$, 1″ in latitude 33° 56′ = 101·739 feet, therefore the difference in latitude = 45″·01; and the latitude of the Observatory being 33° 56′ 3″·25, that of the chimney is 33° 55′ 18″·24. The chimney is south-west of Lacaille's Observatory, about 120 feet, or 115 on the meridian, therefore the latitude of his Observatory was 33° 55′ 17″·11. Lacaille assumed it to be 33° 55′ 15″.

The remainder of the paper is devoted to a description of the heights, distances, and bearings of the mountains about Klyp-Fonteyn, which were ascertained with considerable exactness, and a map constructed, to convey an idea of their form, and probable influence on the zenith sector. The sector, and the repeating circle, for taking the angles, were placed on the corn-floor, on Jacobus Cotsee's foundation, before the foundation of the granary was discovered; and, as the place was comparatively convenient, and the influence of the surrounding masses nearly the same on both, it was considered unnecessary to remove the instruments. The height of the sector above the level of the sea was found, by barometrical comparisons, to be nearly 400 feet.

May 10. The following communications were read:—

Occultations of the Pleiades by the Moon, observed at Ashurst, March 19, 1839. By Robert Snow, Esq.

Occultations of the Pleiades, observed at the Royal Naval Asylum, Greenwich, March 19, 1839. By the Rev. George Fisher, A.M.

On the suspected Variability of the Star *α Cassiopeiæ*. (Extract from a Letter from the President, Sir J. F. W. Herschel, to Mr. Baily, dated Slough, April 28, 1839.)

"My attention was attracted to the star *α Cassiopeiæ* on the 18th of October last, by noticing that it was on that night very decidedly less than the star γ of the same constellation, and that in fact γ was then the chief star. On referring to my father's catalogues of comparative brightness, however, α is found placed above γ. It was, therefore, evident that a change had taken place. On two or three subsequent nights the fact was verified, and other eyes than my own were called in to establish its reality. A considerable succession of cloudy nights intervened before other comparisons were procured; and when a favourable opportunity again occurred, viz. on the beautifully clear night of Nov. 12, the order of magnitude was found restored to that assigned by my father, viz. α, γ, β. On the 27th and 28th of Dec. and the 22nd of Jan. 1839, the observations are positive to this effect.

"In this state it remained till I began to suspect some illusion in the October observations; but, on the 24th inst., being a remarkably clear and beautiful night, γ was again the principal star, and α was inferior not only to that, but to β. And so I observed it to be only about an hour ago. It is true the constellation is now low, but the stars are so near together that the difference of altitude can by no means account for the very marked difference in brightness, though its tendency is certainly in that direction. I should not think of making this observation the subject of a distinct announcement,

were it not that my own attention is necessarily so much distracted from these objects as to make me desirous that some other observer may take up the subject, and verify or disprove the variability of the star in question; and, if verified, assign the period of change.

"I may take this opportunity to point out δ *Orionis* as a star of whose variability I am almost certain."

Extract of a letter from M. Gautier, Director of the Observatory at Geneva, to the President, accompanied with the Observations of Encke's Comet, made at Geneva in the months of October and November 1838.

The observations were made with an equatorial of Gambey, having a telescope by Cauchoix, of 4 inches aperture, and 42 feet in focal length; and the circles, which are 30 inches in diameter, giving by means of verniers the arcs to every three seconds. The instrument required no adjustment during the whole time of the observations, and the comparison of observations of the best known stars showed that it possessed great stability and accuracy of division. The method of observing which was practised does not, however, admit of a degree of precision comparable to that which may be attained when it is possible to employ a wire micrometer illuminated upon a dark field. On account of the extreme feebleness of the comet's light, at least during a part of the observations, a micrometer of this kind could not be used, and in lieu of it a simple cross was employed, formed of small thin plates of silver, about 1′ of a degree in breadth, visible without illumination, with a magnifying power of about 25 times. All the observations of the comet and stars were made by referring each body to this cross, noticing the instant of observation by a sidereal clock, and reading the position of the telescope by the verniers of the two circles. The thickness of the plates of the cross necessarily impairs the precision of the determination, especially for the declinations; but with a little practice and care, values were obtained with sufficient exactness, and of about the same precision as those of comets generally are. The right ascensions were found by taking the differences of the times of observation and the horary angle of the equatorial circle, and the declinations by taking the complement of the polar distances indicated by the declination circle. The advantages of this method of proceeding are, that the observations can be multiplied in a short space of time, and that the best known stars can be taken for a comparison.

Of the three tables which accompany this communication, the first contains all the observations of the comet, in number 189, corrected only for the error of the clock, which, however, is scarcely of any consequence, as it effects equally the observations of the comet and of the stars used for comparison; the second contains the observations of the stars, and their comparisons with the mean positions deduced from the catalogue; and the third contains the reduction of the observations of the comet and their comparison with the ephemeris of Mr. Bremicker.

In comparing the results of the observations of the comet at Berlin and Geneva, there is a sufficient agreement on the whole, although

the positions observed at Geneva are in general a very little advanced in the sense of the comet's motion in respect of those observed at Berlin. M. Gautier thinks the results plainly indicate that a diminution is required to be made in the value of the mass of *Mercury* adopted by M. Bremicker in calculating the effects of the perturbations; and this is also the opinion of Encke: but M. Valz, who observed the comet at Marseilles, from the 23d of November to the 16th of December, is of a contrary opinion. The observations of the latter, it may be remarked, confirm entirely a circumstance remarked on a former occasion by himself, and also by Struve; namely, the contraction of the nebulosity in proportion as the comet approaches to the sun.

On Ptolemy's Catalogue of Stars. By Francis Baily, Esq., Vice-President of the Society.

The catalogue of stars, which goes under Ptolemy's name, will always be interesting to the astronomer, as containing the first record of the state of the heavens. The precise epoch for which it was formed is not clearly ascertained. Ptolemy himself says, that it is reduced to the first year of the reign of Antoninus, which corresponds to the year A.D. 138; but there is some mistake or confusion, on this point, which has led many persons to believe that Ptolemy himself did not actually make the observations from which the catalogue was deduced, but merely reduced, or brought up, a more ancient catalogue of Hipparchus, by means of an erroneous precession, to his own time. It is very evident that the longitudes of all the stars in Ptolemy's catalogue are above 1° too great; but, from what cause this has arisen, it is not my object here to inquire. The point to which, in the present view of the case, I am more desirous of directing attention is, how far the existing editions of that work may be considered as faithful transcripts of the catalogue as it issued from Ptolemy's hands.

This important question can only be decided by a careful examination of various manuscripts, and by comparing the discordant readings with the actual positions of the stars as determined from modern observations. Unfortunately the public have not been in possession of much varied information on this head, the manuscripts hitherto employed having, until lately, been confined to three only; to which two others have been recently added, as I shall presently explain. But all these are, in many cases, so grossly discordant, that it would appear, at first sight, almost a hopeless task to reconcile the different readings that present themselves, or to account for the introduction of so many discrepancies in so small a portion of Ptolemy's great astronomical treatise. The five sources of information here alluded to are,

1st. The Latin translation published by Liechtenstein at Venice, in 1515; the name of the translator is not known, nor is it stated whence the manuscript was obtained. The translation, however, bears internal evidence of having been made from an Arabic manuscript, and throws great light (as I shall presently show) on the subsequent translations and editions from the Greek manuscripts.

2d. The Latin translation made by Trapezuntius (George of Trebizond), and published by Gauricus, also at Venice, in 1528. This trnslation, from which most of the subsequent editions seem to be copied, is said to have been made from a copy of a Greek manuscript which the Abbé Laurentius Bartolinus caused to be made from one in the Vatican library.

3d. The Greek edition published at Basil, in 1538, by Grynæus. It is said that he made use of a manuscript that belonged to Regiomontanus, who had it from the Cardinal Bessarion, and deposited it in the library of Nuremberg. This, however, has been since doubted; and it is not quite certain where the manuscript above mentioned is now to be found. This is the more to be regretted, as it is the only Greek edition extant, except the recent one which I am now about to mention. It is evidently not the manuscript from which either of the preceding translations was made. The work is dedicated to our Henry VIII.

4th. The two other sources are comprised in the Greek edition (accompanied by a French translation), published at Paris, in 1813, by M. Halma. In editing that part of the volume which contains the catalogue of stars, M. Halma availed himself of two additional manuscripts which were in the public library of Paris: one of these he calls the Paris manuscript, which is made the ground-work of his publication, and the other the Florence manuscript. But some other sources must have been appealed to, as he occasionally inserts readings which are not to be found in either of these manuscripts, or in the Basil edition above mentioned.

These several sources of information are all that the public press affords us in our inquiries relative to this interesting subject; but they are lamentably deficient for the purpose. And, as it is probable that the public are not fully aware either of the amount or the frequency of discordance that exists in this matter, I trust I shall not encroach too much on the patience of the meeting by stating, as briefly as possible, the result of my own investigations and researches. Having carefully compared the position of every star, as given in each of the several copies above alluded to, I have found that out of 1028 stars, of which the catalogue consists, there are about 780 (or more than three-fourths) of them that are discordant, either in longitude or latitude; and this, not merely in 10, 20, or 30 minutes, which is no uncommon difference, but sometimes to an amount involving whole degrees. That these errors have arisen mostly from the carelessness of the copyists, and that they may be partly corrected, in some cases, by a reference to the true position of the star, I am ready to admit; but still there are numerous cases where this tentative method will not avail us, and where we are, after all, left in doubt as to the identity of the star, or the true reading of the original numbers.

One source of error I have discovered to be very common, and in the correction of which I have found the translation from the Arabic to be of essential service. It is this. The Greek notation for minutes being denoted by some fractional part of the degree, and such

fraction being expressed by a dash annexed to the letter which in common notation denotes the integer, frequent mistakes occur from the copyist having affixed the dash when it ought not to be inserted, and from omitting it when it ought to have been annexed. Thus 23° is correctly denoted by $\kappa\,\gamma$; and 20° 20′ (or $20°\frac{1}{3}$) is correctly denoted by $\kappa\,\gamma'$: again 34° is correctly denoted by $\lambda\,\delta$; and 30° 15′ (or $30°\frac{1}{4}$) is correctly denoted by $\lambda\,\delta'$. Now it is very readily seen that mistakes of great moment may be made by the omission or misplacing of the dash to the second letter: and here it is that the translation from the Arabic frequently comes in to our aid; for, as their notation was not liable to the same sort of confusion, we are oftentimes led to the true reading by a reference to their copies. I could point out numerous instances of this kind; one, however, will be quite sufficient to illustrate my meaning. The star in *Capricornus*, the fourteenth of that constellation in Ptolemy's catalogue, whose longitude in all the above-mentioned editions, except the first, is said to be 26°, is in the translation from the Arabic stated to be in longitude $20\frac{1}{6}°$, or 20° 10′; which is in fact its more correct value. And it is clear that the erroneous translation from the Greek has arisen from not affixing the dash to the second letter in the expression $\kappa\,s$, which ought to be $\kappa\,s'$. Still there remain numerous other cases where this mode of explication will not avail, and where it would be desirable that other sources of information should, if possible, be thrown open to us: and it is on this point, as I have before alluded, that I am more especially induced to make the present appeal.

Now, it appears that there are several works in this country that might assist us very materially in the elucidation of this subject. In the Bodleian library at Oxford there is a Greek manuscript of Ptolemy, which was presented by Selden; and there is also, in the same library, Bernard's copy of the Basil edition of 1538, wherein he has copied out all the longitudes and latitudes of the stars in the catalogue from this same manuscript. In the library of All Souls' College, it is stated, by Fabricius, that there is the manuscript of a Latin translation from the Arabic; and I understand that there are also manuscript Latin translations from the Arabic at New College and at Magdalen College. There is also a manuscript commentary in Persian, belonging to St. John's College. In the library of the Archbishop of Canterbury at Lambeth there is also a Greek manuscript of Ptolemy. In the library of the British Museum there is an Arabic manuscript, of the date 1218; but I have not been able to find there any Greek copy.

It is evident, therefore, that we have in this country several sources of original information, of which we might avail ourselves, to render the catalogue of Ptolemy more perfect than it is; and, lest it might be supposed that this would be an useless labour at the present day, when the state of the heavens is so much better known, I would remark that it is on this very account that more accurate information is required; since we now know that many minute and gradual changes are going on, which were not suspected or thought of in former times, and which are only perceptible after a lapse of many

centuries. Thus *Sirius* is described in all the original documents that I have seen, as ὑπόκιρρος, *subruffa*, reddish; whereas, at the present day, it is remarkable for its freedom from all colour. Now, this is a point on which Ptolemy could not well be mistaken. Again, a star of the fourth magnitude (the seventeenth in the constellation of *Eridanus*) is clearly laid down by Ptolemy, but cannot now be found; and there are some others, of smaller magnitude, that cannot be identified, according to the positions given in the present editions of the catalogue. But whether these bodies have vanished wholly from our sight, or have been erroneously copied from the original observations, can only be satisfactorily explained (if, indeed, they ever can be) by reference to other authorities. It is needless, however, to dwell further on so obvious a principle.

I had taken the liberty of suggesting to the late Professor Rigaud (a name ever dear to the lovers of astronomy, and more especially to those engaged in historical researches in that science) the propriety of requesting the University of Oxford to print the catalogue of Ptolemy, from the Greek manuscript in their possession: a request which, I understand, was favourably received. We are all sensible of the obligations under which we lie to the University of Oxford, for its noble and spirited conduct, on former occasions, in publishing the works of some of the best ancient authors on scientific subjects, which otherwise might never have seen the light; and certainly not in so splendid a dress. Witness the works of Euclid, Apollonius, Archimedes, &c.; and in more recent times, a continuance of the same liberal and enlightened course on various occasions, in the publication of works that reflect honour and credit on the University, and from which they can never expect to reap any pecuniary benefit.

Since that application, however, was made, the information relative to the additional manuscripts above-mentioned has been obtained; and it may now become a question whether it may not be presumed that a more accurate copy of Ptolemy's catalogue is more likely to be deduced from a careful collation of all the manuscripts within our reach, compared with the several original editions and translations above alluded to, than from the printing and publication of a single Greek manuscript. Should a plan of this kind be attempted, I would propose that a few copies of the Basil edition of 1538 be reprinted (for I fear that the original work is too scarce to be met with in sufficient quantity for this purpose), and distributed amongst those persons who would each undertake to collate such copy with some one or other of the manuscripts in the several archives above-mentioned. This would be no great task or labour to those who feel an interest in the cause, and who are zealous in the promotion of science. Copies even might be sent abroad, to some of our foreign members, residing in places where original manuscripts are known to exist; and who might thus add to the common stock of information. But, whichever course may be adopted, I trust there is no difference of opinion as to the propriety of taking some steps relative to this matter: and I hope that what

I have here stated may induce others to join in carrying so desirable an object into execution.

Mr. Baily stated to the meeting that further accounts had been received from America, relative to the annular eclipse of the sun on the 18th of September last. Professors Henry and Alexander observed it at Princetown College, New Jersey. About eighteen seconds before the formation of the ring, the moon's limb became brightly illuminated. An appearance similar to a row of beads was regarded as the formation of the ring: the drops continued for a second or two. Professor Alexander remarks, that the luminous arch round the moon's dark limb, and the brush of light, were only partially visible in his 4-feet Fraunhofer, with a yellow screen-glass, having a slight tinge of green: but he saw them distinctly in his $3\frac{1}{2}$-feet Dollond with a red screen-glass, for about four minutes after the rupture of the ring: whence it is inferred that the appearance of the beads of light and the dark lines frequently noticed, may be completely modified by the colour, and consequently the absorbing power of the screen-glass through which they are observed.

It was noticed by most of the observers, that before the formation and after the rupture of the ring, the edge of the moon *off* the sun was distinctly visible, and illuminated for some distance within the moon's surface.

CAMBRIDGE PHILOSOPHICAL SOCIETY.

April 29, 1839.—A meeting of this Society was held on Monday evening, Professor Cumming, one of the Vice-Presidents, being in the chair. Mr. Gregory exhibited various photogenic drawings, and described the process by which they were prepared. Professor Sedgwick gave an account, illustrated by various drawings, of the geological structure of Cornwall, according to the new views respecting the place of its rocks in the order of the British strata to which he and Mr. Murchison have recently been led.

May 6.—A meeting of this Society was held on Monday evening, Dr. Graham, the President, being in the chair. Professor Miller made a communication on the Theory of Halos, with particular reference to Fraunhofer's views on that subject. Mr. Green read a note, additional to a former memoir, on the Reflection and Refraction of Light. Mr. Gregory read a paper on Chemical Classification.

May 20.—A meeting of this Society was held on Monday evening, Dr. Clark in the chair. Mr. Ansted made a communication on the Tertiary Formations of Switzerland. Mr. Green read a memoir on the motion of light through crystallized media. Mr. Whewell read a note respecting the working of his Anemometer since his memoir on that subject read May 1, 1837. The Anemometer had since that time been in action at the Society's house, and at the Cambridge Observatory; but in consequence of the instruments being frequently repaired and improved, the observations were fre-

quently interrupted. The observations for the months of July and August 1838 were, however, represented by comparative diagrams, which were exhibited to the Society. From this comparison it appeared, that the form of the line representing the course of the wind as registered by the instrument at the two places is nearly identical, thus proving the consistency of different instruments of this construction with one another. The scale of the two instruments appeared to be different, nearly in the ratio of 2 to 1, but no direct comparison of the scales had been attempted. It was stated also, that, during 1837 and 1838, observations had been made with Mr. Whewell's Anemometer at Edinburgh by Mr. Rankine, and expressed in a diagram (according to the method recommended by Mr. Whewell) in the 14th volume of the Edinburgh Transactions. Observations with this instrument have also been made at Plymouth, and reduced by Mr. Southwood (of St. Peter's College), by whom also the diagrams for Cambridge were constructed. Mr. Whewell stated, in conclusion, that there is every reason to believe, from the results hitherto obtained, that if any person with sufficient leisure were to take up this subject, it will reward him by leading to the knowledge of important meteorological facts and laws.

AMERICAN PHILOSOPHICAL SOCIETY.

July 20, 1838.—The Committee appointed on the communication of Dr. John Locke of Cincinnati, read at the last meeting, [consisting of Messrs. Peter S. Duponceau, R. M. Patterson, and J. Saxton,] made the following report, which was adopted.

"The Committee to whom was referred the communication of Professor John Locke, of Cincinnati, report, that it gives the details of a series of experiments, made for the purpose of determining the magnetic intensity and dip for certain positions in Ohio. For these experiments he had furnished himself in London with the best apparatus, and had vibrated there two needles of the form recommended by Hansteen, and one in the form of a small flat bar. Five months afterwards, namely, on the 17th of January, 1838, he again vibrated these needles at Cincinnati, and found the ratio of horizontal intensity at the former place to that at the latter, as follows: by needle, No. 1, as 1 to 1·1624; by needle, No. 2, as 1·1639; by No. 3, as 1 to 1·2037. Of these results the author prefers the last; inasmuch as the magnetism of needles is liable to decrease, but not to increase.

"On the 20th of August, 1837, he made experiments with his dipping needle, to determine the dip at Westbourn Green, near London, the mean of which gives 69° 23′·3.

"On the 26th of November, 1837, the mean of a series of experiments made at Cincinnati, in lat. 39° 6′ N., and long. 84° 27′ W., gave the dip = 70° 45′·75.

"At Dayton, Ohio, in lat. 39° 44′ N., and long. 84° 11′ W., the dip was found to be 71° 22′·75 on the 26th of March 1838.

"At Springfield, Ohio, in lat. 39° 53′ N., and long. 83° 46′ W., the dip was found on the 29th of March 1838, to be 71° 27′·375.

"At Urbana, lat. 40° 03′ N., long. 83° 44′ W., March 30, 1838, the dip was found = 71° 29′·94.

"At Columbus, the seat of government of Ohio, lat. 39° 57′ N., long. 83° W., April 3, 1838, the dip was found = 71° 04′·875.

"The interest of this paper is much increased by the circumstance that no accurate experiments on the intensity and dip of the needle have heretofore been made in the United States, west of the Alleghany mountains.

"The Committee conclude their report, by recommending that Professor Locke's communication be printed in the Society's Transactions."

Sept. 21, 1838.—The Committee on the solar eclipse of the 18th of September, made a report in part, comprising the observations made at Philadelphia, the principal results of which are as follows:

The observations made at Philadelphia are fifteen in number. A list of observers, telescopes, &c. is given in the following table. The correction in the third column is to be added algebraically to the latitude of the place of observation, to obtain that of the State-House, +39° 56′ 58″. The correction in the fourth column is likewise to be added to the local longitude in time, to obtain that of the State-House, $-5^h\ 0^m\ 39{\cdot}2^s$.

No.	Observers.	Reduction to Latitude of State-House.	Reduction to Longitude of State-House.	Focal length in feet.	Maker of Telescope.	Description.	Screen Glass.	Estimated Power.
1.	E. J. Beans	″ −70·0	+1·70	2·5	Unknown.	Spy-glass	smoked	15
2.	Wm. Penn Cresson...	− 1·8	+5·20	2·5	Jones.	Achromatic	red	30
3.	Prof. W. R. Johnson	− 1·8	+5·20	3·5	Dollond.	do.	do.	100
4.	George M. Justice...	−10.0	+2·86	2·5	Jones.	Gregorian	do.	80
5.	E. O. Kendall	−10·0	+2·86	2·5	Plössl.	Dialytic	green	50
6.	Joseph Knox	−21·0	+1·39	3·5	Dollond.	Achromatic	red	80
7.	Isaiah Lukens.........	− 9·0	+0·86	1·8	Plössl.	Dialytic	yellow	20
8.	Thomas M'Euen......	+ 0·4	+2·33	2·5	Dollond.	Achromatic	red	60
9.	Prof. Roswell Park	− 6·5	+1·30	2·5	do.	Gregorian	do.	50
10.	Dr. R. M. Patterson	− 1·1	+1·20	5·0	do.	Equatorial	do.	100
11.	W. H. C. Riggs	+ 0·4	+2·33	3·5	do.	Achromatic	do.	50
12.	Samuel Sellers	− 7·5	+0·05	2·5	Jones.	do.	do.	40
13.	Tobias Wagner	−10·0	+2·86	3·5	Dollond.	do.	do.	80
14.	Seares C. Walker ...	−10·0	+2·86	5·0	Tulley.	do.	do.	100
15.	William Young	+21·0	−1·39	7·0	Hilcomb.	Herschelian	do.	200

A. *Beginning.* Professor Johnson noticed dark indentations for eight seconds after the first disturbance of the limb.

B. Arch of faint light, with speck or brush in centre, round the moon's limb beyond the cusps; brush or blaze in centre, between cusps, extending outwards about two digits. One cusp broken at end presenting a bright bead.

C. Arch of light much increased in brightness, the brush or blaze, at first in the centre, now extends from cusp to cusp; radia-

tion outwards, nearly three digits; cusps distant 30° on sun's limb, a broken point or bead at each end. This phase noted as that of the formation of the ring by Nos. 1, 2, 3, 4, and 11.

D. *Formation of ring or instant of osculation of limbs.* This phase noticed as the approach of two sharp well-defined points to a contact, by Nos. 5, and 15. It was observed at the instant when the cusps, apparently 20° of the sun's limb apart, suddenly united by the extension of four or five luminous beads, or rounded portions of the sun's disc, by Nos. 3, 4, 8, 9, 10, 11, 13, and 14.

E. Omitted in the table. This letter refers to the time when the dark lines, described by Van Swinden and Baily, should have appeared. They were not seen by any observer, though carefully searched for.

F. Perfect ring, the beads of light having united, or run into each other suddenly.

G. Counterpart of E, not observed though looked for.

H. *Rupture of ring,* counterpart of D. Took place at a point and so noted by all the observers.

I. Appearance of beads, five or six in number, extending from cusp to cusp.

K. Counterpart of C, in every respect.

L. Counterpart of appearance just preceding C. Brush or blaze of light, narrowed down to a small space, 3° or 4° on the moon's border, extending outwards 2½ digits; cusps still broken as seen by most of the observers. Nos. 5 and 15, however, saw no irregularity of cusps, no beads of light.

M. Final disappearance of arch of faint light, with brush of light extending beyond the middle, having previously become very faint. This phænomenon observed with great care and certainty by No. 10.

Phases observed, in mean times of the Places of Observation.

No.	A. h. m. 3 13	B. h. m. 4 30	C. h. m. 4 31	D. h. m. 4 31	F. h. m. 4 31	H. h. m. 4 35	I. h. m. 4 35	K. h. m. 4 35	L. h. m. 4 35	M. h. m. 4 41	N. h. m. 5 45	O. h. m. 5 45	P. h. m. 5 45
			s.			s.							
1.			3·9			28·4							
2.	s.		—70			27·8	s.	s.			s.	s.	
3.	10·7	...	10·5	s.	15·5	23·5	27·5	29·0	...	...	4·2	12·2	
4.	7·4	...	6·3	12·8	...	27·3	...	...	...	...	...	11·3	
5.	8·3	...	...	10·9	...	28·4	...	...	...	...	...	12·9	
6.	12·8												
7.	...	...	...	21·7	...	...	...	36·2					
8.	*3·0	...	...	18·1	...	29·1	...	...	...	...	...	13·2	
9.		s.		19·1	...	29·1	...	...	...	s.			s.
10.	7·0	39·1	...	19·1	...	30·1	...	...	...	23·1	...	16·1	19·1
11.	7·3	39·2	2·3	16·3	...	29·4	...	36·3	...	...	...	7·8	
12.	6·0	...	...	16·0	...	31·0	...	...	...	...	...	16·0	
13.	6·1								s.				
14.	5·6	36·7	...	15·6	23·0	28·0	29·5	37·0	42·0	...	10·0	13·0	16·0
15.	...	...	...	12·9	...	...	...	38·9	...	...	...	15·0	

* Doubtful.

N. Appearance of dark lines extending into the sun's disc, noticed by Nos. 3, 4, 10, and 14. The time noted by Nos. 3 and 14, as the end of the eclipse.

O. *End of Eclipse,* inferred by each observer from his notes.

P. Final disappearance of the dark lines, the sun's disc having resumed its natural shape, Nos. 3, 4, 10, and 14, inferred the time of O as at some instant intermediate between N. and P. The time of external contact difficult to determine on account of this irregularity.

For the convenience of computors the local times above given have been reduced to their corresponding value for the State-House by E. O. Kendall, by means of his formulæ, in vol. xx. of the Journal of the Franklin Institute, p. 125, which gives the following values for the variation of the local times of the several phases, for a small variation of terrestrial latitude or longitude as follows:

	Beginning.	Ring.	End.
Variation for + or north $1''$ terr. lat.	$=-0{\cdot}0397$	$-0{\cdot}0382$	$-0{\cdot}0343$
Variation for + or east 1^s of terr. long. in time..	$=+1{\cdot}2600$	$+1{\cdot}1400$	$+0{\cdot}9925$

The means of his results for the State-House giving to each observation its proper weight in mean time of the State-House, are,

Beginning..................	$3^h\,13^m\,10^s{\cdot}06$
Formation of ring	4 31 18 ·76
Rupture of ring	4 35 31 ·35
End..	5 45 15 ·46
Duration of eclipse	2 32 5 ·40
Duration of ring	0 4 12 ·59

Dr. Patterson read a paper by Professor Charles Bonnycastle, of the University of Virginia, containing "Notes of Experiments made August 22 to 25, 1838, with the view of determining the depth of the Sea by the Echo."

This paper, which was not offered for publication in the Society's Transactions, states that the generally received notions in regard to the intensity of sound in water, and the distance to which it is conveyed, had suggested to Mr. Bonnycastle some years ago, the idea that an audible echo might be returned from the bottom of the sea, and the depth be thus ascertained from the known velocity of sound in water. The probability of this view was deemed at least sufficient to justify an experiment; and accordingly the Navy Commissioners authorized the construction of the necessary apparatus, and Captain Gedney, of the U.S. brig Washington, attached to the coast survey, volunteered his services, and the use of his vessel, and authority to this effect was liberally granted by the Secretary of the Treasury, Mr. Woodbury.

The apparatus, which is fully described in Mr. Bonnycastle's paper, consisted first of a petard or chamber of cast iron $2\frac{1}{2}$ inches in diameter, and $5\frac{1}{4}$ inches long, with suitable arrangements for firing gunpowder in it under water; secondly, of a tin tube, 8 feet long and $1\frac{1}{4}$ inch in diameter, terminated at one end by a conical

trumpet-mouth, of which the diameter of the base was 20 inches, and the height of the axis 10 inches; thirdly, of a very sensible instrument for measuring small intervals of time, made by J. Montandon, of Washington, and which was capable of indicating the sixtieth part of a second. Besides these, an apparatus for hearing was roughly made on board the vessel, in imitation of that used by Colladon in the Lake of Geneva, and consisted of a stove-pipe, $4\frac{1}{2}$ inches in diameter, closed at one end and capable of being plunged four feet in the water. The ship's bell was also unhung, and an arrangement made for ringing it under water.

On the 22nd of August, the brig left New York, and in the evening the experiments were commenced. In these, Mr. Bonnycastle was assisted by the commander and officers of the vessel, and by Dr. Robert M. Patterson, who had been invited to make one of the party.

In the first experiments, the bell was plunged about a fathom under water and kept ringing, while the operation of the two hearing instruments was tested at the distance of about a quarter of a mile. Both instruments performed less perfectly than was expected, the noise of the waves greatly interfering, in both with the powers of hearing. In the trumpet-shaped apparatus, the ringing of the metal, from the blow of the waves, was partly guarded against by a wooden casing; but, as it was open at both ends, the oscillation of the water in the tube was found to be a still greater inconvenience, so that the sound of the bell was better heard with the cylindrical tube. At the distance of a quarter of a mile, this sound was a sharp tap, about the loudness of that occasioned by the striking the back of a penknife against an iron wire; at the distance of a mile the sound was no longer audible.

In the second experiments, the mouth of the cone in the trumpet apparatus was closed with a plate of thick tin, and both instruments were protected by a parcelling of old canvas and rope-yarn, at the part in contact with the surface of the water. In these experiments the cone was placed at right angles to the stem, and the mouth directed towards the sound. The distances were measured by the interval elapsed between the observed flash and report of a pistol. At the distance of 1400 feet, the conical instrument was found considerably superior to the cylindrical, and at greater distances the superiority became so decided, that the latter was abandoned in all subsequent experiments. At the distance of 5270 feet, the bell was heard with such distinctness as left no doubt that it could have been heard half a mile further.

The sounds are stated in the paper to have been less intense than those in air, and seemed to be conveyed to less distances. The character of the sound was also wholly changed, and, from other experiments, it appeared that the blow of a watchmaker's hammer against a small bar of iron gave the same sharp tick as a heavy blow against the large ship's bell. It is well known that Franklin heard the sound of two stones struck together under water at half a mile distance; yet two of the boat's crew, who plunged their heads

below the water, when at a somewhat less distance from the bell, were unable to hear its sound.

On the 24th of August, the vessel having proceeded to the Gulf-stream, experiments were made with the view for which the voyage was undertaken; that is, to ascertain whether an echo would be returned through water from the bottom of the sea. Some difficulties were at first presented in exploding the gun under water, but these were at length overcome. The hearing-tube was ballasted so as to sink vertically in the water. The observers then went with this instrument to a distance of about 150 yards from the vessel, and the petard was lowered over the stern, about three fathoms under water, and fired. The sound of the explosion, as heard by Mr. Bonnycastle, was two sharp distinct taps, at an interval of about one-third of a second. Two sounds with the same interval were also clearly heard on board the brig; but the character of the sounds was different, and each was accompanied by a slight shock. Supposing the second sound to be the echo of the first from the bottom of the sea, the depth should have been about 160 fathoms.

To ascertain the real depth, the sounding was made by the ordinary method, but with a lead of 75 pounds weight; and bottom was distinctly felt at 550 fathoms, or five furlongs. The second sound could not, therefore, have been the echo of the first; and this was proved on the following day, by repeating the experiment in four fathoms water, when the double sound was heard as before, and with the same interval.

The conclusion from these experiments is, either that an echo cannot be heard from the bottom of the sea, or that some more effectual means of producing it must be employed.

Oct. 5, 1838.—The Committee on the solar eclipse of the 18th of September, made a further Report in part.

This portion of the report embraced the observations made in the vicinity of Philadelphia, of which the following are the principal results, arranged in the order in which they were received, and with one exception, in mean time of the place of observation; the longitudes being reckoned from Greenwich.

No. 16. by Robert Treat Paine, Esq., at the west front of the Capitol, Washington. Latitude 38° 53′ 23″, as determined by Mr. Paine with his Troughton's sextant. Longitude 5h. 8m. 8s. west. With $3\frac{1}{2}$ feet equatorial, green screen glass. Time by three chronometers, regulated by eastern and western altitudes of sun and stars with his Troughton's sextant.

Beginning	3^h	6^m	$9^s{\cdot}58$
Formation of ring	4	24	28·15
Rupture of ring	4	30	18·55
End	5	39	54·89
Duration of eclipse	2	33	45·31
Do. of ring	0	5	50·40

"The ring formed instantaneously, and broke nearly so. No beads were seen, nor the dark lines mentioned by Mr. Baily, nor

the light round the moon, although all were looked for. No distortion of the moon's limb could be seen, and the cusps of the sun, before the ring formed, were as sharp as needles."

No. 17, by Lieut. Gilliss, U.S.N., at the Marine Observatory, Washington City, N. 8″, W. 0·08^s in time from the Capitol, with a 3½ feet achromatic, green screen glass, power 50. Astronomical clock regulated by a five feet transit instrument.

Beginning	$3^h\ 6^m\ 10^s{\cdot}4$
Formation of ring	4 24 28·4
Rupture of ring	4 30 18·9
End	5 39 56·4
Duration of eclipse	2 33 46·0
Do. of ring	0 5 50·5

"At beginning of eclipse, limbs sharp and well defined. The same at formation and rupture of the ring, only in the former the light seemed to flash round the moon's limb." Two detached arched portions of the ring were seen separated from the cusps, "while the space between presented points of light (beads) only."

No. 18, by Prof. Elias Lumis, at the Observatory of the Western Reserve College, Ohio. Latitude 41° 14′ 42″ N. Longitude $5^h\ 25^m\ 35^s$ W. With a five-feet equatorial, mounted on a stone pier under a revolving dome, with yellow screen glass, power 150 nearly. Astronomical clock regulated by a 30-inch transit circle by Simms.

Beginning $14^h\ 27^m\ 26{\cdot}7^s$ sidereal time.
Other phases lost by clouds.

Nos. 19 and 20, by J. Gummere and his son S. J. Gummere, at the Haverford School Observatory, Chester County, Pa. Latitude 41° 1′ 12″ N. Longitude $5^h\ 1^m\ 16^s$ W. With two 3½ feet telescopes by Tulley, with red screen glasses, power 75 nearly. Astronomical clock regulated by a Dollond's portable transit instrument.

Beginning	$3^h\ 12^m\ 17^s{\cdot}2$
Formation of ring	4 30 29·2
Rupture of ring	4 34 44·8
End	5 44 28·7
Duration of eclipse	2 32 11·5
Do. of ring	0 4 15·6

Arch of faint light with brush in centre, seen before the formation of the ring. Arch seen after rupture, brush of light not recollected. Formation and rupture of the ring, by broken portions of the sun's border, several in number, not round like beads, but arched portions of the ring. These continued several seconds, and then suddenly united in the first instance, and separated in the last, without, however, exhibiting the dark lines figured by Baily.

Nos. 21 and 22, by Charles Wister and his son Caspar E. Wister, at the Observatory of the former, Germantown. Latitude 40° 1′ 59″. Longitude $2^s{\cdot}7$ in time west of the State House, with 2½ and 2 feet Gregorian reflectors. Astronomical clock regulated by a 3 feet transit instrument.

	C. Wister.	C. E. Wister.
Beginning	$3^h\ 12^m\ 55^s{\cdot}4$	$3^h\ 12^m\ 54^s{\cdot}4$
Formation of ring	4 31 9·4	4 31 8·4
Rupture of ring	4 35 18·4	4 35 18·4
End................	5 45 8·4	5 45 7·4
Duration of eclipse....	2 32 13·0	2 32 13·0
Do. of ring	0 4 9·0	0 4 10·0

"The lucid points and dark intervening spaces corresponded closely to Baily's description,"

No. 23, by John Griscom. Latitude 9″·7 N. Longitude 0·3^s in time west of the Observatory of Haverford School. With a 3½ feet Dollond achromatic, power 80.

Beginning....................	$3^h\ 12^m\ 18^s{\cdot}6$
Formation of ring	4 30 31·6
Rupture of ring (not reported)	
End	5 44 26·6
Duration of eclipse	2 32 8·6
Do. of ring (not reported).	

No. 24, by Prof. James Hamilton of Burlington, New Jersey. Latitude 40° 5′ 10″ N. 69·1^s in time east of State House, Philadelphia. With a five feet achromatic, power 80, clock regulated by equal altitudes with a sextant.

Beginning....................	$3^h\ 14^m\ 23^s{\cdot}7$
Formation of ring	4 32 32·6
Rupture of ring................	4 36 19·6
End	5 46 8·5
Duration of eclipse	2 31 44·8
Do. of ring	0 3 47·0

"The phases of the ring are the perfect formation and perfect rupture, without reference to beads. No dark lines seen."

LXXIII. *Intelligence and Miscellaneous Articles.*

PROCEEDINGS OF THE COMMITTEE OF COMMERCE AND AGRICULTURE OF THE ROYAL ASIATIC SOCIETY OF GREAT BRITAIN AND IRELAND.

THE first Number of the Proceedings of this Committee has appeared, and the valuable information which this pamphlet contains, fully bears out the expectations which its formation justified us in entertaining. The reports of this Committee cannot fail to possess high interest for the merchant and manufacturer, as pointing out the much neglected resources of our extensive Indian possessions, as indicating new sources of long-known articles of commerce, and describing new substances well adapted as substitutes for many of those well known, and at present imported from foreign countries.

The Proceedings contain abstracts of a series of papers on various important subjects; amongst which are cotton, rice, silk, caoutchouc, tanning substances, dye-stuffs, medicines, oils, and oil-seeds; these are principally by Dr. Royle, the Secretary of the Committee; Mr.

E. Solly, Jun., their Chemical Analyser; Mr. Southey; Mr. Gibson, of the Bombay Medical Establishment; Mr. Heath, of the Indian Iron and Steel Company; Mr. Hughes, of Tinnivelly; Mr. Sievier, Superintending Manager of the London Caoutchouc Company; Drs. Cantor, Lush, and Geddes, &c.

We look forward with great interest to the publication entire of the papers which have been communicated to this Committee; and we hope that it will meet with that encouragement which the importance of its objects entitles it to expect.

SEPARATION OF ETHYLE (ETHULE, ETHEREUM, = 4 C + 5 H.) BY C. LÖWIG.

Small pieces of potassium are placed in a glass tube 3 to 5 lines wide containing chloride of ethyle; a powerful action ensues, and the metal becomes covered with a white crust, which should be broken up so as to cause a fresh metallic surface to be exposed to the action of the fluid. The mixture soon begins to boil: a tube bent at right angles should be fixed in the mouth of the large one, to connect it with a receiver kept cool by a freezing mixture; chloride of ethyle unmixed with any other product distils over. Ultimately, if sufficient chloride were present, all the potassium becomes converted into the white crust; if this be exposed to heat, it gives off an inflammable gas, becomes black, and by exposure to air undergoes rapid combustion.

When the white crust is placed in water it dissolves, hydrogen gas being evolved; on agitating the watery solution with æther, and decanting the æthereal solution, a volatile, oily fluid is obtained by evaporating it *in vacuo* at a low temperature. This fluid burns with a vivid flame, has a peculiar soap-like odour, and very acrid taste.

On submitting the white powder obtained by the action of potassium on chloride of ethyle to analysis by combustion with oxide of copper, it yields,

0·870 carbonic acid = 0·2405 carbon,
0·450 water = 0·0500 hydrogen;

and 100 parts will consequently consist of

Carbon	82·79
Hydrogen	17·21——100·

or,	4 atoms carbon	305·74	or,	4 atoms carbon	24
	10 atoms hydrogen	62·40		5 atoms hydrogen	5
	atoms of ethyle	368·14		atoms of ethyle	29
	equivalent to	83·05 carbon,			
		16·95 hygrogen,			
		100·			

which approaches very closely to the result of experiment.

From these experiments Löwig concludes, that by the action of potassium on chloride of ethyle, chloride and *ethylide* of potassium (*Aethylkalium*) are formed, the latter combination being decomposed

by the action of water, setting free the ethyle, either pure or as an hydrate. Löwig further remarks, that a combination of ethyle with sulphur, analogous to that with potassium, exists, and may be prepared by adding an alcoholic solution of sulphuret of potassium to chloride of ethyle; in a short time chloride of potassium is deposited, and the sulphureous combination left in solution.

Poggendorff, *Annalen*, 45, p. 347. Oct. 1838.

SEPARATION OF PHOSPHORUS FROM ITS OXIDE. BY M. BOTTGER.

All chemists who have prepared oxide of phosphorus by the usual method of burning phosphorus under water by a current of oxygen gas, are aware that a considerable quantity of free phosphorus adheres mechanically to the oxide thus obtained; and that it is removed with extreme difficulty, however cautiously the distillation may be conducted. This difficulty is derived from the circumstances, that if a current of gas free from oxygen be not continually passed into the distillatory apparatus, and the heat be not carefully regulated, the oxide of phosphorus decomposes very readily, while too low a temperature does not completely expel the phosphorus. Some good methods of obtaining oxide of phosphorus have lately been published, among others, that of M. Leverrier, by chloride of phosphorus, which leaves nothing to be wished for; but M. Bottger thinks that his process is probably more easy of execution. He has found that sulphuret of carbon is of all known bodies the best solvent of phosphorus; in fact, contrary to the generally admitted opinion, that this substance can dissolve only 8 parts of phosphorus, he has proved that it can dissolve 20 at a mean temperature, whereas the oxide of phosphorus is not at all acted upon by it; the method which he proposes is the following: put the impure oxide of phosphorus, obtained by combustion, into a large bottle, pour sulphuret of carbon upon it, with an equal measure of absolute alcohol: cork the bottle, and shake it well for about a minute, then allow the oxide to subside, and pour off the phosphorized liquor; repeat this operation with a fresh portion of the sulphuret and alcohol, and then put the oxide of phosphorus on a filter, and wash it first with alcohol and then with water; after this dry it by exposure to the air, or what is still better under a receiver over sulphuric acid.

The product thus obtained appears to possess all the properties of pure oxide of phosphorus; when heated in the air, it resists combustion at a high temperature. When mixed with chlorate of potash it produces a strongly detonating powder, violent explosion taking place even during mixture, without employing any considerable pressure. It is desirable to determine by analysis whether the oxide obtained by this process is similar to the yellow oxide procured by that of M. Leverrier. According to Pelouze the oxide obtained by combustion and purified by distillation consists of 3 ats. phosphorus + 1 at. oxygen, while that procured by the decomposition of the chloride consists, according to Leverrier, of 4 ats. phosphorus + 1 at. oxygen.—*Journ. de Phar.* Feb. 1839.

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